THE

Medical and Phyfical Journal.

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To Dr. BRADLEY.

DEAR SIR,

THE operation of Tapping during Pregnancy, has, I perceive, engaged the attention of fome of your ingenious Correfpondents. It is, doubtlefs, a fubject of confiderable intereft and importance, in fo far as the life, not only of the fœtus, but even of the mother, may depend on the greater or lefs fkill and dexterity with which it is performed. If a Cafe in which the water was drawn off from the cavity of the abdomen five times, from the fixth week of conception to within twenty-five days of delivery, without inconvenience either to the mother or fœtus, fhould be deemed worthy of a place in your valuable Journal, the following, which occurred fome years ago in my practice, is at your difpofal. Befide furnifhing an ample field for fpeculation to the ingenious Phyfiologift, it may poffibly ferve as a guide to the inexperienced practitioner, fhould fimilar inftances fall under his care. It is intended for publication as foon as my avocations will permit, with many other, I truft, interefting Cafes, in an Appendix to a work on Hydrothorax, which has been for fome time nearly ready for the prefs.

On the 13th of June, 1791, I was defired to vifit Mrs. C. of ——— Hall, in Effex, a married woman, in the 34th year of her age. I found her labouring under confirmed genuine afcites, unaccompanied with the flighteft traces of anafarca in any part of the body. She had in a certain degree the characteriftic emaciation of this difeafe, but not, its general concomitant, debility. The appetite, bodily ftrength, and indeed all the bodily functions, with an exception or two that fhall be prefently noticed, were unimpaired. The eye and countenance too, had loft none of their wonted animation, for fhe was remarkable for the vivacity and gaiety of her difpofition. The urine, fhe imagined, was fomewhat diminifhed in quantity, but natural in appearance. The origin of the dropfical affection was dated about two years back; and it was attributed to an injury fhe fancied fhe received in her laft and only lying-in, five years before: This opinion feemed countenanced by the catamenia

catamenia having been more or leſs irregular, both as to time and quantity, ever ſince. She was ſuppoſed to have inherited a ſcrophulous diatheſis, ſeveral of her family having died of what was pronounced *Phthisis Scrophulosa.*

Various remedies had been uſed without any advantage; ſhe had been tapped three times; and the water was rapidly accumulating, though evacuated only four days before.

After attentively conſidering all the circumſtances of the Caſe, I had no heſitation in aſcribing the dropſy to diſeaſe of the right ovarium: This I found to have been the opinion of Dr. Orme, of London, who was conſulted in the early period of the diſorder; and it gained additional confirmation from the facts communicated by Mr. Gretton, a reſpectable practitioner of Colcheſter, who had always attended her. He aſſured me, that on evacuating the water an evident fullneſs or tumefaction could be felt in the right ſide, apparently in the region of the ovarium.

The common remedies were adminiſtered in various combinations for ſome weeks, but without any benefit. They could not, however, be ſaid to have had a fair trial, having been taken only in ſmall quantity, and very irregularly, owing to a peculiar irritability of the ſtomach to almoſt all diuretic medicines. Though every precaution was taken by the addition of the moſt grateful aromatics, and ſmall doſes of opium, yet they were ſeldom retained long enough to act upon the habit. As I could not poſſibly be aware of what was aſcertained ſome weeks afterwards, that my patient was pregnant, this peculiar ſtate of the ſtomach was at the time a ſource of ſurpriſe and embarraſſment to me, for the appetite and digeſtion were good.

On my viſit, the 9th of Auguſt, when the operation was judged neceſſary, for the firſt time ſince my attendance, I found Mrs. C.'s mind ſtrongly impreſſed with the belief that ſhe was pregnant. This ſhe grounded on the abſence of the catamenia the two laſt periods, and on her having the uſual ſymptoms, eſpecially ſickneſs, and ſometimes vomiting, in the morning.—After a lapſe of five years ſince her lying-in, with every reaſon to believe the exiſtence of diſeaſe of one ovarium ſo extenſive as to be the ſole cauſe of the dropſy, apparently extending its morbid influence to the uterus itſelf, (if a conſtant irregularity in its functions ever ſince were to be regarded as criteria to judge by) beſides this organ having been ſurrounded by, or floating in, a body of water for more than two years, I could not be ſuppoſed to aſſent readily to her opinion. No inconvenience occurred either during or after the operation. When the water was evacuated, the hypogaſtric region was fuller than uſual; ſo much ſo, as to induce me now to think her opinion well

well founded. The enlarged ovarium was diſtinctly felt on examination, but no tumour perceived externally.

In order to avoid repetition, the different times at which the operation was performed, and the quantity of water drawn off each time, are ſpecified in the annexed Table, and only ſuch circumſtances mentioned as ſeemed worthy of being recorded.

Toward the middle of Auguſt, every doubt as to her pregnancy was removed, for ſhe quickened, the motion of the fœtus having been diſtinctly and repeatedly felt by her.

On the 25th of September, tapping being again deemed expedient, additional precaution was neceſſary, with a view of avoiding the hazard of abortion, or of wounding the diſtended uterus, now emerged above the brim of the pelvis. I directed a cordial draught with twenty-five drops of laudanum to be adminiſtered half an hour before the operation; and the patient being placed in the uſual poſture, I endeavoured to preſs the body of the uterus as far back as poſſible with both my hands, at the place where the puncture was to be made, while, with my fingers, the integuments were at the ſame time protruded forwards. By theſe precautions, the diſtance between the parietes of the abdomen at this place was increaſed, and the integuments rendered more tenſe. In the mean time, Mr. Gretton meaſured on the trochar the length that was judged merely ſufficient to enter the cavity of the abdomen; and by firmly placing the fore finger of the right hand as a guard upon it, prevented any more entering than was abſolutely neceſſary. The puncture being then dexterouſly made, and the ſtiliette withdrawn, the water was evacuated more gradually, the mouth of the canula being now and then plugged up by a piece of wood prepared for the purpoſe; and a more regular and uniform preſſure was preſerved by means of a proper bandage. No untoward circumſtance occurred either during or after the operation.

Before the whole of the water was removed, the motion of the fœtus was not only felt with great force againſt the hand, but diſtinctly ſeen thro' the integuments and flannel bandage.

Nov. 9th. She had inceſſant ſickneſs and vomiting, with ſevere uterine pains which threatened abortion, the whole of this morning. The breathing was much interrupted, and ſhe ſuffered great inconvenience from diſtention of the integuments of the abdomen. As theſe evidently proceeded from inordinate preſſure on the diaphragm, uterus, and other abdominal viſcera, the operation having been too long delayed, ſo they all ſpeedily vaniſhed on the evacuation of the water. Some part of the fluid was tinged with blood, and conſiderable delay and inconvenience were experienced from the preſſure of the uterus

against the canula; yet ſhe made no complaint either during or after the operation.

Dec. 30th. Bore the operation better than uſual, being in high ſpirits the whole time, though the motion of the fœtus occaſioned her much uneaſineſs, and though greater delay was experienced than before. During the latter months of pregnancy, ſhe became, as may naturally be imagined, extremely heavy and unwieldy, eſpecially for ſome days before the two laſt tappings: yet her ſtrength, appetite, and natural vivacity continued unimpaired to the laſt; and on the 24th of Jan. 1792, ſhe was delivered of a ſtrong, full-grown boy, after a very eaſy and quick labour, before Mr. G. her *accoucheur*, or myſelf, arrived. The boy is now living, and is remarkably healthy and ſtrong of his age.

Her recovery was unuſually rapid, though there were ſeveral pints of fluid in the cavity of the abdomen at the time of her delivery, and though it accumulated ſo quickly afterwards as to require being drawn off in eleven days.

It may be worthy of remark, that after this period 'till the ſudden change which preceded and occaſioned her death, ſhe affirmed, her general health appeared rather to improve than otherwiſe; a circumſtance to be wondered at, conſidering the immenſe quantity of matter that had been for a great length of time collected within the body, as will be ſeen in the Diſſection. It ſerves however, I think, to illuſtrate and confirm a fact in Pathology which is now generally admitted, that what is called well digeſted or concocted pus, whether it be ſhut up in a cavity, expoſed to the air, or carried to the habit through the medium of the abſorbents, is productive of no inconvenience either to the ſurfaces on which it reſts, or to the conſtitution at large: So aware is every judicious ſurgeon of this, that he cautiouſly avoids expoſing the tender and irritable granulations of healing wounds and ulcers by wiping their ſurfaces too clean. It ſeems at the ſame time to prove, what is I think likewiſe allowed, and what I have more than once obſerved, that the ovaria are endowed with comparatively leſs acute feeling than perhaps any other part of the body; for in the preſent inſtance the mechanical ſtimulus of diſtention occaſioned no uneaſineſs. What other part or organ would have ſuffered ſo ſerious an injury, and ſo great a degree of diſtention, without ſerious inconvenience both local and conſtitutional?

It is intereſting too, to obſerve the influence preſſure had on the accumulated fluids during the laſt month, as might naturally be expected, when the uterus was attaining its utmoſt extent of ſize, and when, conſequently, the preſſure on the numberleſs

numberless exhalant and absorbent mouths opening into the cavity of the abdomen was the greatest, the effusion was proportionably retarded. This did not occur so uniformly and regularly in the preceding months, for the obvious reason that it must have been influenced by various causes and circumstances which did not now exist. But while the mechanical influence of pressure, muscular and arterial action, in promoting absorption and lymphatic circulation, is to be admitted to a certain extent, yet I am far from assenting to the notion of some modern Physiologists, that they are the sole agents concerned in producing these effects. This opinion has, I perceive, been recently advanced by a very ingenious Correspondent in Vol. vi. p. 457,* of the Medical and Physical Journal, but is not supported by any evidence tending to establish its validity. Among many arguments, and, I think, facts that might be adduced in proof of the contrary; viz. that the absorbents are endowed with active, living, muscular powers, and that they *actively* absorb as well as transmit their respective fluids, in a certain degree totally, independent of mechanical impulse, I shall only mention one, which appears to me to afford decisive evidence in its favour. By the use of Digitalis various cavities are often speedily unloaded of enormous quantities of serous fluid, when there is positive proof of diminished, muscular, arterial, and every other impulse or action, in consequence of the powerfully sedative and debilitating effects of certain quantities of this herb; and this has happened sometimes from the cavity of the abdomen, where the greatest pressure that could be borne proved of no avail.

The operation was in this case performed in the usual place on the left side. Would a puncture with a lancet at the umbilicus, as recommended by the learned President of the London Medical Society, Dr. Sims, be a more simple, easy, or safe mode of drawing off the water in similar cases? I am induced to think it would not. It might, probably, succeed equally well in the early stages of pregnancy; but the pressure of the uterus, after the fourth month, against the opening thus made, would very soon be such as to require the introduction of a female catheter, or similar instrument, to push it backwards; and it is well known, that a round instrument passed into an opening

* These are the authors words, in a note subjoined to his interesting communication: "The phrase *lymphatic transmission*, is here substituted for the ordinary term *absorption*, it being my opinion that the lymphatic vessels do not in any instance *actively absorb*, but *passively admit* the redundant, exhalant, interstitial, or extravasated fluids forced into their *permanently open orifices* by arterial and muscular impulse."

opening made with a lancet would occasion much pain and inconvenience; more so, I am persuaded, than our patient experienced. For these reasons, the method adopted with so much success in the Case of which the history has been given, claims, I think, a decided preference. A large female catheter was occasionally introduced through the canula.

Dates of the different Tappings, &c.

When tapped. 1790. First time.	No. of pints.		Days intervening.		Pounds weight.
Aug. 23.	11	—		—	12½*
1791.					
March 23.	25	—	212	—	28½
June 9.	33	—	78	—	38
Aug. 9.	33	—	61	—	37
Sept. 25.	37	—	33	—	42
Nov. 9.	38	—	45	—	41
Dec. 30.	12¼	—	51	—	14
1792.					
Jan. 24. Delivered.					
Feb. 4.†	27	—	36	—	30½
March 2.	18½	—	26	—	20
April 11.	44	—	40	—	51
May 9.	44	—	28	—	49½
June 6. ‡	36	—	28	—	40
June 11. Died.					
	8) 359 (				404
	Gallons, 44 : 7 Pints.				

Dissection.

Permission having fortunately been obtained to inspect the body, Mr. Gretton very obligingly attended to conduct the dissection. On opening the abdomen, a sac of considerable size was observed in the right side of the hypogastrium, and nearly filling the right Iliac region. This was found to be the right ovarium attached to the uterus by a thick neck, and adhering anteriorly

* This was not weighed, only guessed at; but all the other times the water was accurately weighed and measured, either by, or in the presence of the husband of the lady; and from the period of my attendance, in my presence also.

† About an ounce of thick good pus issued from the opening, at the close of the operation.

‡ The evening after the operation of this date, a train of symptoms came on, which left no room to doubt the gradual approaches of the fatal event which soon followed.

teriorly to some extent to the peritoneum. It was partially collapsed, owing to about two pints of its contents, a thick, brown, well digested pus, having escaped through an opening on one side, sufficiently large to admit three fingers into the cavity of the abdomen. Neither the uterus nor left ovarium shewed any marks of disease. The os tincæ seemed to be closed by a gelatinous like substance, which induced me to think a second impregnation had taken place. But my much esteemed friend and Preceptor, the celebrated Professor Monro, to whom the parts were transmitted, after having been carefully removed and preserved in spirits, assured me that this was not the case.

On dividing the sac, it was very thick but of a soft and spongy texture, extremely irregular on its internal surface, and containing upwards of three pints of matter similar to that found in the cavity of the abdomen. The external surface of the intestines, both great and small, was very considerably inflamed, verging in some places on gangrene; and it was smeared all over with the matter which escaped from the abscess, of which the greater part was contained in the pelvis. The liver was softer in its texture; and its connecting ligaments more relaxed and elongated than natural. The other viscera, as well as those of the thorax, were sound. The head was not opened.

The inflammation of the intestines, not the mere rupture of the abscess, was, without doubt, the immediate cause of death; and that this was occasioned by the morbid stimulus of the purulent discharge from the sac seems equally probable; which shews, that though good pus be productive of no inconvenience to the surfaces by which it is secreted, it is highly noxious to others.

Had this Case occurred to me at an earlier period, before suppuration, and impregnation had taken place, and before the stomach had, by sympathy with the uterus, in consequence of the latter, acquired such inordinate susceptibility to medicine, I think it probable the event might have been more favourable. I am inclined to draw this inference from the fortunate results of two similar Cases which fell under my care not long afterwards.

Mrs. R. the subject of the first Case, was a married woman of forty-six, the mother of a numerous progeny. She had confirmed ascites, without any anasarcous swelling, of three years standing, evidently arising from diseased and considerably enlarged ovarium. The abdomen was so large and tense, and she suffered so much inconvenience, that I advised tapping before I put her on a course of medicine. To this, however, she positively refused her assent. She was completely cured, and is now in perfect health, by a combination of calomel, squills,

crystals

cryſtals of tartar, other diuretics, and ultimately tonics, perſevered in for ſome weeks. She firſt conſulted me in Auguſt, 1792, and ſhe has had no relapſe ſince.

The ſubject of the ſecond, was a Mrs. B. the wife of the Rev. Mr. B. late of Bury. She had a conſiderable, enlargement of one ovarium for many months before any ſigns of dropſy appeared. For this Mr. Cline was conſulted. He pronounced upon the Caſe, and preſcribed for it, with his uſual accuracy and judgement. When my attendance was firſt deſired, the 31ſt of Auguſt, 1792, the abdomen was conſiderably diſtended with water; and the leg and thigh correſponding with the diſeaſed ovarium were anaſarcous to a high degree: This laſt, indeed, began to appear for ſome months before there was any ſerous effuſion in the abdomen. She was likewiſe perfectly cured by a ſimilar plan; and before ſhe left this country, about three years afterwards, had two children. I have not heard of her lately.

I am, &c.

Sudbury, Feb. 6, 1802. L. MACLEAN.

To Dr. BRADLEY.

DEAR SIR,

I Received, a few days ago, from my brother, who is attached to the Embaſſy at Amiens, an Addreſs of a very intereſting nature, which was preſented to the Marquis Cornwallis. The original Inſtrument, and a Tranſlation which I have made, are encloſed, that you may publiſh either, or both, in your valuable Journal.

This Communication is not made on account of the additional proofs it contains in favour of the Inoculation of the Vaccine, all oppoſition to this great Diſcovery ſeems now to be ſilenced; like the doctrine of the Circulation of the Blood by the immortal Harvey, it is already eſtabliſhed: But it muſt give pleaſure to every humane mind, to be informed of the zeal with which this important improvement is received and propagated in France, where it is even made the ſubject of Congratulation to a Britiſh Embaſſador. It appears, that the name of JENNER is repeated there with the juſt eulogiums that are due to the man, who, practiſing medicine in a remote part of the country, had the ingenuity to raiſe out of an obſcure rumor, a moſt uſeful invention; who had the perſeverance to demonſtrate its innocence by convincing experiments; who, by ſuperior

perior knowledge, refuted the objections of his enemies, and corrected the errors of his friends; and, lastly, who, scorning to make a secret of his discovery for his private advantage, had the virtue to publish it openly, for the Benefit of Mankind.

I am, &c.

Grosvenor Street, Feb. 5, 1802. JAMES MOORE.

LIBERTY. EQUALITY.

Amiens, Frimaire 29th, 10th year of the Republic, (16 *October*, 1801.)

From the MEMBERS of the JURY of HEALTH, and the MEDICAL COMMITTEE of the Department of the SOMME, to His EXCELLENCY the MINISTER PLENIPOTENTIARY of ENGLAND for the CONGRESS at AMIENS.

My Lord,

THE Jury are constantly occupied with whatever relates to the preservation of Man. Vaccination has justly called forth their particular attention; and in the course of a year, a great variety of experiments have therefore been made here, upon more than six hundred persons. The first Magistrate of this Department has given every encouragement to our trials; and the Discovery which has been made in your country, has been stamped in our's with the seal of Infallibility. The Vaccine is now proved to be a Preservative against the Small-Pox. This can no longer be doubted. England, my Lord, has the honour of this Discovery; we have received the Vaccine from your Physicians. The Friends of Science never interrupt their fraternal intercourse; and while their Governments wield the thunder of War, to decide their political contests, men of Literature always remain in Peace. The Vaccine which has been sent here, has taken root; we have repeated the experiments of the immortal JENNER, and we have found them correct. Other experiments which we have instituted, confirm the justness of his conclusions, as well as what has been promulgated by Pearson, Simmons, and Woodville; and the fortunate discovery made in Gloucestershire shall triumph for ever here. No improvement was ever assented to with so much unanimity, in spite of envy and avarice. But, my Lord, none of our experiments were more decisive than that which we shall relate to you; not so much for our gratification, as for the benefit of the art of Medicine and of Humanity; and in laying this before you, we wish that the Glory which

which has been acquired may be transmitted to the Discoverer.

The 25th of last Germinal, (16 April) three infants at the Hospital St. Charles, named Duneuf Germain, Fracaster, and Pisson, who had before this been vaccinated with success, were now inoculated with Small-pox matter: This produced no effect, and the triumph of the Vaccine was published. However, the incredulous and ill intentioned have always objections to raise, and calamities to prophecy; they asserted, that the preventive power was only temporary; that Jenner's proofs were not sufficiently convincing; that that Physician lived far off; and, in short, they denied every thing. But, upon the 25th Vendemiere last, (17th October) the Jury of Health at Amiens, inoculated these three children again. Every precaution was taken to prevent mistakes, and they were in a manner deluged, rather than inoculated, with the most virulent Small-pox matter. Still, however, these children did not contract the Small-pox; they were not even disordered; and now, at the expiration of sixty days, they continue in perfect health. After this, who will dare to assert that the Vaccine is not a Preservative against the Small-pox? There is no man, after such authentic proofs, who would not blush to deny it. Will it yet be said, that the preservative power continues only for a time? Why then, after six complete months, should the second Inoculations with Small-pox fail? Why did they produce no more effect than the first? And, why is not the period pointed out, when the preservative power shall cease?

This is a difficult problem for the Detractors of Vaccination to solve. But they have changed their ground; they no longer deny the preservative power which has been demonstrated, they entrench themselves behind certain accidents which they pretend have been occasioned by the Vaccine; they have forged and published a hundred falshoods, which have been quickly detected: The Jury of Health, my Lord, have the satisfaction of perceiving that they are too strong for the Impostors: They are now of opinion that they ought no longer to reply to their interested opponents, who, although they are Physicians, tremble at every discovery favourable to humanity; they do not deserve the honour of being confuted; they will never acknowledge their defeat, for they are neither influenced by truth nor the glory of the Art of Medicine, but are instigated solely by their deity, Avarice.

Accept, my Lord, our homage, and this account of the last experiments we have made, as an offering which we have the honour to present you with; they confirm, decisively, the admirable invention for which we are indebted to the Medical Science

Science of ENGLAND. We have already declared, that the French Physicians have never ceased to consider themselves as brothers to your Physicians; and, when you have finished your important labours at Amiens, the two nations will love each other reciprocally; and France and England, glorying in their valour, united by mutual esteem, shall command Repose to the rest of the World.

We have the honour to salute your Excellency with profound respect.

SEVILLE.
CORNET.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

HAVING read in your Medical and Physical Journal, some Queries respecting the *imperfect* or *spurious* Vaccine pustule, together with some observations on the supposed cause of the occasional deviation from the genuine character of the disease, I beg leave to offer a few remarks from the result of my own experience, with an ardent wish that they may throw a ray of light on a subject which so intimately concerns the dearest interests of humanity.

At the commencement of my practice in this branch of the Medical Profession, I generally took the Vaccine Virus for inoculation after the pustule was arrived at maturity, and the *areola* completely formed, because I then found the fluid in the greatest quantity; and as I had frequently from one to two hundred persons of a day apply to be inoculated, the *quantity* of virus became a material object to me. The consequence was, that in some instances the operation failed of producing any, and in others but an imperfect effect. I was the more misled by observing the Vaccine matter to retain its entire fluidity till a late stage of the disease, even (as subsequent experiments have convinced me) after its active or specific properties were entirely lost. When I had a sufficient number of patients to furnish me with *early* virus, I inoculated upwards of 200 persons with it *in a recent fluid state*, and had the satisfaction to find that they were all infected, and went through the complaint in the most perfect and satisfactory manner. I was now induced to believe that using *late* virus was the principal, if not the sole cause of the spurious pustule, and accordingly made it a rule

rule *never to inoculate from an arm after the eighth or ninth day, or when the efflorescence surrounding the pustule was formed to any considerable extent*; and by a rigid observance of this law, I have seldom failed of producing the perfect Vaccine from the first inoculation: But as some few instances of a spurious pustule have occurred in my practice, notwithstanding the above precautions, we must not regard the use of late virus as the *only* inefficient cause, although I have reason to believe it to be the most frequent one.

An imperfect pustule now and then arises from the use of virus previously dried too hastily by the fire. The heat, if it does not destroy the action of the Vaccine fluid altogether, frequently decomposes it, and renders its preservative powers doubtful; for it is on the complete pustule alone we are to depend for security against a future attack of the Small-pox.

In two instances I have seen the *perfect* and *spurious* pustule co-existent on the same person, for which I do not presume to assign a cause. There are, no doubt, as in the Small-pox, if we may reason from analogy, many circumstances that may conspire to suspend the action of the Vaccine virus, or render its effect incomplete. These occurrences, however, are rare; but whenever they do appear, I would recommend a re-inoculation of the patient. But from whatever source the spurious pustule may arise, there is this satisfaction, that it is very easily distinguished from the perfect disease by those who have paid any attention to the Vaccine practice. The features of the genuine disease are strongly marked, and require but little discernment to be familiarly acquainted with them.

Persons who have previously had the Small-pox are sometimes susceptible of the local action of the Vaccine virus, and a spurious pustule is produced, altho' it oftener fails of exciting any effect whatever. I have seen a few instances where the perfect disease has been produced under similar circumstances, one of which I shall mention. I repeatedly inoculated myself with Vaccine matter, but never could excite the least inflammation till I had repeated the operation fifty times. The fiftieth inoculation produced a spurious pustule, which was premature in its progress, and terminated the eighth day in a brown polished scab. A drawing of this pustule was taken by an ingenious artist (Mr. William Cuff) and is now in the possession of my uncle, Dr. Jenner. Two cases of perfect pustule after the Small-pox have come under my observation: One was a boy, inoculated by Mr. Foster, Surgeon, of Thornbury, in Gloucestershire. The other happened to myself from the following circumstance. A young lady of a timid disposition, dreading the operation of inoculation, wished to be shewn in what manner

ner it was performed. I shewed her by making a slight puncture on the back of my hand with a lancet, on the point of which was a little dried Vaccine virus. About the fifth day I felt an itching, and observed a slight vesication, which I readily perceived to be the Vaccine. This increased in size till the ninth day, when it was surrounded by an effloresence, and then declined in the usual way. The axillary glands were slightly affected, and the pustule was smaller than what is commonly excited on the arm. On the eighth day, I took some matter from my hand and inoculated a young man, who has at this time the Vaccine disease in the most unequivocal manner.

I shall not enter into any argument on the propriety or impropriety of the term *spurious*. I have here used it as it has been generally accepted. I shall conclude with a hope that the Medical Gentlemen of this Empire, and others whose authority and example may have influence with the lower orders of society, will cordially co-operate in adopting and supporting a practice which has for its object the total extinction of the Small-pox. The present age will then triumph in having vanquished that foe to health, to beauty, and to life, while its name only will be known to posterity.

If you think the above worthy a place in your Journal, you will oblige me by inserting it.

I remain, &c.

Burbage, Wilts, Jan. 20, 1802. G. C. JENNER.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN your 34th Number, is recorded a professional Controversy on the subject of *Apoplexy*. As a prelude to the projected discussion of this disease, it may be admissible to lament the want of candour which is so frequently manifested in Medical Controversies. Illiberality is the bane of science, the disgrace of individuals, and a deformity too frequently attached to the professors of our art.

From the first blush of the narrative before me, it is impossible not to censure the conduct of the *Surgeon*. I choose to give full credit to his own representation, and upon that alone I ground my verdict. His reasons for *not consulting with Dr. Langslow* are veiled in impenetrable mystery; but, whatever they may be, I am persuaded, that a disclosure cannot do honor

to

to his judgement or to his feelings. It is acknowledged that a fellow creature, labouring under a most dangerous malady, was denied the benefit of a *consultation*; and till I am better instructed, I must believe, that a valuable lady would have perished, had her life depended upon a friendly professional intercourse between *Dr. Langslow*, and *one of her attendant surgeons*.

Mr. Primrose's conduct was, apparently, unexceptionable.

On the face of the narrative, there is nothing ungentlemanlike, or inconsistent with professional etiquette, on the part of *Dr. Langslow*. So much for the Controversy.

I shall now attempt an Inquiry into the nature of *Apoplexy*; and I sincerely hope, that, through the medium of your excellent Journal, the disease will undergo a thorough examination. I flatter myself too, that, in the Inquiry to be instituted, the cause of science alone will be the object of dispute. If the chief duty of our art be an alleviation of the ills connected with our nature; and if that end be atchieved, it is not benevolent to be jealous of the means.

Much has been written about the nature and cure of *Apoplexy*; and a man who is wedded to no system, finds it difficult to form his opinion; because, happily for our species, this disease does not occur so frequently, as to furnish any individual practitioner with much actual observation. That he may settle his faith, he must recur to the records of others. He must collate, arrange, and compare the histories so detailed. Above all, he must advert with particular attention to appearances after death. From inquiries so conducted, I am persuaded that inferences may be established, approximating to the truth, nearly enough for successful practice.

There is in authors a very striking coincidence of description of the *living* and *dead phænomena* of *Apoplexy*. The extreme points being thus settled, it is to be expected that the intermediate steps should be traced with the most perfect agreement. That the curative indications of *apoplexy*, however, are, in different hands, repugnant to each other, is sufficiently evident from the present Controversy. The school division of this disease into *sanguineous* and *serous*, is now, I believe, very generally, and I think justly, exploded; for were the distinction demonstrated to be true, it does not admit of a beneficial application in practice. I shall therefore dismiss this distinction without a comment, and consider *Apoplexy* as arising from *something* COMPRESSING *the brain, and thereby destroying the energy of that organ.* This definition, I imagine, will embrace almost every form of the disease. I do not affect to mark with precision the degrees of disorganization attached to those cases of *Apoplexy* which terminate in recovery; but in fatal cases, where an inspection

ſpection has been permitted, it is certainly true, that a *turgescency* of the vaſcular ſyſtem, or an *effuſion* of fluid matter, have been conſtantly demonſtrated.

If thoſe who have recovered from, and thoſe who have periſhed by, *Apoplexy*, have exhibited features of diſeaſe preciſely ſimilar, I think it is probable that the favorable and the fatal caſes have had their origin in the ſame cauſe, more or leſs powerfully applied. A brainular plethora is rendered probable in favorable caſes of the diſeaſe, by its being demonſtrated in caſes which have terminated fatally; but whether this *turgeſcency* be excluſively attached to the *arterial system*, or to the *system of recurrent vessels*; or whether *effuſion* (when it occurs) be *arterial* or *venous*, are queſtions which will, for ever, perhaps, demand a doubt; nor do I think it would be of high practical intereſt to put the matter beyond ſuſpicion. The multiplied evidence of Diſſection will prove, that in the ſame patient the veſſels of the head have been found, ſome in a *turgid*, others in a *ruptured* ſtate. If this be true, it ought not merely to influence but to fix the indication of cure; for whether *turgeſcency* be preſent without *effuſion*, or there be a combination of both, I am perſuaded that the ſame curative inſtruments ought to be employed.

In the treatment of *every Caſe of Apoplexy*, I preſume upon the preſence of COMPRESSION; for altho' I admit that a ſlow and inſidious diſeaſe may aſſail the brain itſelf, and may eventually induce *Apoplexy*, yet, as ſuch morbid phænomena are placed beyond the reach of our diagnoſtic art, a perfect knowledge of their exiſtence cannot *practically* avail us.

I ſhall never be able ſatisfactorily to prove it, but I am ſtrongly inclined to believe, that, in all caſes of convaleſcent *Apoplexy*, there is no material *rupture*, but merely a *turgeſcency* of the *vascular system*. I ground this belief upon a conviction that a *ſudden* effuſion of any *large quantity* of fluid upon the brain, is incompatible with animal life.*

It muſt be admitted, that patients having a large collection of fluid in the ventricles of the brain, have lived for a conſiderable time; but it ſhould be obſerved, that that fluid has been ſlowly effuſed or ſecreted, and the parts gradually adapted to its preſence. *Hydrocephalus* is, I believe, never cured; for were it poſſible to remove the water by a wiſh, I imagine that the collapſe would be fatal to the patient: It is ſcarcely poſſible, I think, under circumſtances of *confirmed Hydrocephalus*, for the moſt ſanguine practitioner to expect a gradual abſorption.

I do

* Slight effuſions have often occurred without a fatal event. See Doctor Baillie, &c.

I do not pretend to trace the *arterial system* of the brain to its minuteſt ramifications, or to mark with preciſion the point at which they become returning veins; but, from the collated evidence before me, I am inclined to believe in *arterial* not *venous effuſion*. The enunciation of this opinion, however, cannot affect the indication of cure; and I would induce a diminution of brainular plethora by *local and general bleedings, bliſters, and purgative clyſters.*

The exhibition of *Emetics*, is, in my mind, a practice highly dangerous; nothing, ſurely, can be leſs indicated. The affection of the ſtomach, which firſt ſuggeſted their employment, is very certainly a *ſymptomatic* diſeaſe only; for in all injuries done to the brain, of whatever nature they may be, there is uniformly an affection of the ſtomach: But, with the exception of injury ariſing from *Apoplexy*, I do not know that an *Emetic* was ever thought of to cure any *idiopathic* brainular diſeaſe.

Original thinking is a rare quality; and if this be admitted, I can explain why a great man ſhould be copied, even in his *errors*: But it is not ſo eaſy to ſay, why the expanded mind of a *Fothergill* ſhould miſtake an effect for its cauſe. The ſanction, however, of that illuſtrious obſerver, has long ſince ceaſed to operate upon the practice of men of ſcience; for a moment's reflection muſt convince every one, that, were the doctrine of the ſtomach's mechanical preſſure true, no diſeaſe would be ſo common as *Apoplexy*; and no diſeaſe would be ſo eaſily, and ſo immediately, remedied. There needs no ſpecification of the means. Every man who has treated *Apoplexy*, muſt have witneſſed inſtant relief by the operation of *bleeding*; would this have happened, had the diſeaſe been conſtituted by *diſtenſion of the ſtomach?* After a *full meal*, it is true, that *Apoplexy* very frequently occurs; but this proves nothing more than that the ſtimulus of the food, by increaſing arterial impetus, operates as an exciting cauſe of the diſeaſe.

If the phænomena of *Apoplexy* have their origin in *turgeſcency*, nothing is ſo likely as *vomiting* to produce *rupture* and *effuſion*; and if thoſe ſymptoms be preſent, nothing is ſo likely as *vomiting* to increaſe both. In fine, I can ſcarcely ſuppoſe any practice fraught with greater danger than the adminiſtration of *Emetics* during the attack of an *Apoplexy*; and their employment has been reprobated by the higheſt profeſſional authorities: I cannot quote greater than the juſtly celebrated *Morgagni*, and the enlightened *Cullen*.

I am, &c.

Bradford, 15 Feb. 1802. GEO. MOSSMAN.

Mode

Mode of managing Ocular Inflammations;
By Dr. WHYTE.

THAT expoſure of the naked eye to the intenſe heat and vivid rays of a nearly vertical ſun, ſhould in ſome caſes occaſion an expanſion of the humours and dilatation of the veſſels of that delicate organ, is in the nature of things not to be doubted.

Where the conſtitution is naturally delicate, where it has been formed in another climate, and is not yet ſufficiently aſſimilated to the new one, ſuch expanſion and dilatation will, on the requiſite expoſure, more certainly enſue.

To guard againſt ſuch expoſure conſtitutes therefore a material part of ocular prophylaxis. It is to be effected by ſhading the eyes from the influence of the vertical ray, by means of broad brimmed hats, long vizored helmets, or what would anſwer beſt of all, the tamata of Taheite, a garland compoſed of palm leaves with a plated projecting front piece; this of all coiffures is the moſt eminently ſuitable, becauſe it does not debilitate the head by exceſſive heat, as do the ponderous helmets of European ſoldiers, or the equally ponderous turbans of pious Muſſelmen.

Neither the cocked hat of modern Europe, nor the mouchoir ſo generally worn by the lower claſſes of the French and Spaniards, as well at home as in their American poſſeſſions; nor the turban of the ſwarthy inhabitants of Africa or Aſia, afford any ſhade or protection to the eyes.

Accordingly, all thoſe enumerated claſſes, in every variety of climate and ſoil, are exceedingly liable to ocular inflammations, and to their remote conſequences, ſpecks and cataracts.

In ſome of thoſe countries too, the influence of the ſolar ray is much increaſed by inhabiting a houſe having white-waſhed walls, or where the eye of luxury is dazzled and deſtroyed by the luſtre of elegant but ſuperfluous chandeliers, or by the reflexion from a multiplicity of ſplendid mirrors. Where ſuch cauſes have alone operated, and application has been made for medical aſſiſtance in the firſt ſtage of the diſeaſe, it is for the phyſician or ſurgeon to determine whether the diſeaſe he is about to treat conſiſts in a mere dilatation of the humours, or in an inflammation of the tunics, or whether it is a mixed caſe, and both circumſtances are combined.

In the firſt ſpecies there is frequently no perceptible inflammation; but, by an enlargement of the aqueous humour, and the anterior ſection of the orbit, the focus of concentration falls behind the retina. An indiſtinct image is formed upon it

even in day-light, while towards evening the lucid rays proving ſcanty and feeble, ſeem blended and confuſed, and viſion is completely interrupted.

I am acquainted at preſent with a caſe of this kind, which was induced by the patient indulging himſelf with an afternoon's nap, in the open air, on the rock of Gibraltar, laying on his back, and with his eye-lids conſiderably open.

That this imprudent expoſure did not at the ſame time occaſion an inflammation of the blood veſſels that ramify and run along the ſurface of the tunica albuginea, depended on the abſolute tone and ſtrength of thoſe veſſels, and on their being relatively ſtronger, and leſs expanſible, than the ſubjacent tunics or contained humors.

This man recovered without having had recourſe to medical aſſiſtance.

By avoiding for ſome time the ſtimulus of light, or, more correctly ſpeaking, the expanſive power of heat, the humors inſenſibly collapſed, and the tunics reſumed their original and healthy tone.

Had this perſon, however, been a patient of mine, I would have aſſiſted the ſlow progreſs of Nature, by keeping the ball of the eye conſtantly moiſtened with a cloth or rag dipped in cold water, or ſome gently aſtringent collyrium, as aqua litharg. acetati, aqua zinci, vitriolati, &c.

I would have alſo touched the ball of the eye, morning and evening, or oftener, with ſome aſtringent and ſtimulating tincture, as that of cinchona, or of opium; or, to ſuperſede every other remedy, and ſtrike at once to the root of the diſeaſe, I would have pierced through the tunics with a couching needle, and entering the poſterior chamber of the aqueous humor by an inciſion parallel to and behind the iris, permitted an outlet proportioned to the exiſting expanſion.

No danger need be apprehended from this operation; it is even in this manner I am accuſtomed to extract the cataract. It is a mode that poſſeſſes many advantages. I have performed it frequently, ever with impunity, and often with ſucceſs.

When, from the operation of light and heat alone, the veſſels of the tunica albuginea have alone ſuffered, a ſteady and conſtant employment of collyria, in the extenſive manner recommended, with the diurnal or more frequent application of ſome ſtimulating tincture, will in moſt caſes, where the eye labours under no conſtitutional diſeaſe, and we have been conſulted at an early period, effect in very few days a complete cure, while avoiding of the exciting cauſe will ſecure againſt relapſe.

The momentary pain ariſing from the application of the ſtimulating tincture is often exceedingly acute, yet ſo ſenſible are

thoſe

those who have once experienced it, of its great utility, that the moment I enter some ships where, from incorrigible negligence in not wearing hats or bonnets, this disease is exceedingly frequent, I am surrounded by a circle of soldiers' wives and children entreating to be touched.

I may likewise add, that when leeches can be procured we should do well to apply them in some cases, repeating them as circumstances may require, and applying them as near as possible to the seat of the disease.

In such cases, when leeches cannot be obtained, we may open the jugular vein, or, still better, the temporal artery.

In severe cases I have sometimes scarified inflamed eyes, but after scarification ecchymosis often accedes, and the eye may remain blood-shot, a disagreeable circumstance, for weeks together.

Besides, the application of stimulating tincture and astringent collyria has proved, in my practice, so invariably beneficial as to supersede, in my opinion, every other mode of treatment.

If I hesitate to deform the eye by scarifications, the most indispensible necessity could alone determine me to disfigure the face by disagreeable and disgusting vesicatories.

Sometimes, when from the extreme sensibility of the visual organ, and its vicinity to the primum mobile, the primum mobile itself is affected, I am taught by the common rules of prudence to obviate danger, and afford relief by an immediate diminution of the circulating mass, moderate or copious, and repeated or not, according to the urgency of existing circumstances.

It is almost unnecessary to add, that the state of the bowels must be likewise attended to.

So much for those inflammations of the eyes that originate from improper exposure to heat and light alone.

The inflammations so frequent in Syria and Egypt have often another and more pernicious source.

By means of proper coiffures we can protect the most tender of organs from the meridian rays of a scorching sun; but what human invention, what undiscovered amulet, can secure the eye of him who inhabits a wilderness of sand, from the intrusion of its minute and insinuating particles, when that wilderness is, like the waters of the ocean, agitated by the winds?

Of one thing however we may rest assured, that till the irritating particles are fairly expelled, totally dissolved, or completely encysted, no mode of treatment can operate a cure.

Nature, studious for the safety of all her productions, has been eminently provident of this the most exquisite and beautiful of all her works. She has formed the eye-lids to secure the

the included orbs against the impulse of foreign bodies; and the tears, secreted for the purpose of moistening the eye, and maintaining it in a condition suitable to the performance of its peculiar functions, are on the smallest irritation from a foreign body, secreted in increased quantity, and assist in the solution or expulsion of so dangerous an inmate.

It is the business of the philosophical practitioner to second the efforts of Nature, and endeavour to improve upon her curative indications.

In some countries it is a common practice among the peasants to attempt the removal of motes in the eye by wiping the ball gently with a feather, or employing a damsel to skim it with her tongue; but both modes are rude, and frequently ineffectual. It is more proper, in my opinion, to tread in the footsteps of Nature, and endeavour to cleanse the eye by copious ablution, by a forcible and well directed stream, which pervading every recess, shall in its progress sweep every foreign particle before it. Accordingly, in ocular inflammations arising from this source, or where such a source is at all suspected, I enjoin the eye to be instantly syringed.

Not unfrequently the patient is sensible of the expulsion of the irritating particle during the operation. Sometimes, however, it only shifts its seat, and one or more repetitions are required. In every other respect the inflammation is to be treated as if proceeding from the influence of heat and light alone.

Should any gentleman do me the honour to make trial of the methods I have here recommended, I flatter myself he will find his account in it, and neither have occasion to accuse me of presumption, nor himself of temerity.

Postscript to Dr. Whyte's *Paper on Ocular Inflammations.*

Since writing the preceding account of my mode of treating ocular diseases, I begin to think better of scarification, and to practise it oftener.

I have even met with several cases where, I apprehend, suffusion would have been the consequence of its omission.

I have also seen, since writing the original paper, some Essays on Ophthalmia in the Egyptian Decade, with which I have been favoured by Sir Sidney Smith, and am much surprised that in the enumeration of remedies by one of their principal physicians, scarification is not so much as mentioned.

It is observed indeed by Citizen Bruant, in a different paper, that the natives employ, with much advantage, topical blood-letting at the external canthus, by which, I suppose, he means scarification; but, he adds, that the French could not, or

or had not yet adopted it. Citizen Bruant ſuppoſes that ſome *ophthalmies* proceed from bilious accumulations in the primæ viæ.

If the eye is ſufficiently prediſpoſed, or the fever ſufficiently violent, this may no doubt happen.

This ſpecies, as he chuſes to term it, he treated moſt ſucceſsfully by emetics and purgatives.

General phlebotomy, he ſays, was contra-indicated in this ſecond ſpecies by the preſence of bile; and in the firſt, or local ſpecies, ariſing from duſt, &c., by the debility induced upon the troops by the hardſhips of a nine year's war.

Like preceding medical conjurers, he ſurpriſes us with ſtill a third ſpecies, ariſing, he ſays, from nervous irritability.

The number of different and diſcordant ſubſtances that have been uſefully employed as collyria, demonſtrates to me that they all act upon one common principle, viz. the principle of ſtimulation. Still I cannot place perfect confidence in Dr. Savareſi's obſervations, when he informs us, in one page, that powdered ſulphat of alumine (common alum), invariably produced ophthalmia; and, in a ſubſequent one, that by blowing a powder, compoſed of it, ſugar candy, and nitrated kali (nitre), upon the ball of the eye, he has invariably ſucceeded in the removal of incipient ſpecks.

Savareſi informs us, that of one thouſand patients whom he treated, two only became totally blind, and two loſt an eye.

This was great ſucceſs; and much certainly depended on an early application.

He frequently applied bliſters to the nape of the neck, and always uſed ſtrong ſtimulating collyria.

His cures were, however, exceedingly protracted, being in general from three weeks to two months; and might, I apprehend, have been much ſhortened had he occaſionally ſcarified, as I now do; and had he touched the ball of the eye, once or twice a day, with a hair pencil dipped in ſome ſtimulating tincture.

Although the French have had ſo excellent an opportunity, they do not appear to have acquired a very accurate knowledge of the nature and cauſes of ocular inflammation. In attributing the diſeaſe to this or that ſaline powder, they ought to have reflected that any powder finding admiſſion into the eye, will there produce irritation and pain, and, finally, more or leſs inflammation, till ſuch time as its particles become encyſted, or until they are diſſolved or neutralized, or expelled by the lachrymal ſecretion.

If nitre is found to be leſs irritating than alum, we may preſume it to be more ſoluble. And if any powder is more prejudicial

judicial than another, I believe it to be lime. The Egyptian masons have not the art to avoid its influence, and the French physicians report that most of them are blind. But why contend concerning saline powders, when a single grain of sand, of which there would not appear to be any scarcity in the lower parts of the Egyptian atmosphere, will sufficiently explain the phenomenon; and when, in a thousand other cases, unguarded exposure of the eye to intense solar heat will not only furnish sufficient explanation, but the disease may be often traced to such a source.

Dr. Savaresi talks of sthenic and asthenic ophthalmia; but I disagree with him and many others on this subject; so much so, that I do not even believe there is in existence such a thing as sthenic topical inflammation. There may exist a general increase of vascular action; but I contend that no accumulation can take place in any individual part or organ, unless such part or organ has, constitutionally or incidentally, less strength, or tone, or resistance, absolute or relative, than the other parts where no such accumulation has acceded. If the local predisposing debility, inherent or induced, is sufficiently great, ophthalmia may be excited without any increase of general vascular action. An increase of general vascular action may sometimes be necessary to excite topical inflammation; but the vessels of the part must at the time possess less relative tone, otherwise they would not yield to the distending power. Such local and predisposing debility is produced by no means sooner or so much as by heat. Hence exposure of the eye to the direct influence of the solar rays, will ever predispose to ophthalmia; and, where the predisposition is sufficiently great, the disease will be excited without any ostensible cause, and without any increase of vascular action.

There may be more or less decrease of tone in the inflamed part; a decrease that is recoverable, and one that is not.

That sthenic topical inflammation, properly speaking, does not actually exist, would appear, not only from theoretic reasoning, but is confirmed by the present improved practice of the English surgeon, who applies tonics and astringents to every external part that is inflamed. Could the physician act similarly with internal inflammations, we might entertain some hope of the universal panacea being at last discovered.

The reasoning I have employed is consonant to the established laws of hydrostatics, and to those of the animal œconomy; consequently, topical tonics are indicated in all cases of ophthalmia; and in many, general debilitating powers.

I agree with the French physician, that exposure to nocturnal dews may serve as an exciting cause; and I apprehend I furnish

furniſh a more definite idea of the phenomenon when I ſtate, that, by expoſure to nocturnal cold, the fluids, receding from the major part of the ſuperficies, are forced to concentrate themſelves in that part or organ, internal or external, laboring under the greateſt abſolute or relative debility; — in this caſe, the eye, which it is therefore of much conſequence to ſhade from the ſun. But, in ſhading the eye, I do not conceive that there is an equal neceſſity to have the head covered. It is recorded by an ancient hiſtorian, (Herodotus, I believe) that on the fields where battles had been fought, the head of an Egyptian could be diſtinguiſhed from that of a Parthian. The latter, which had been habitually inveſted with the many-folded tiara, was thin, light, and ſemi-tranſparent. The former had gone bare headed when alive; and his ſkull, hardened and baked in the ſun, was thick, opake, and ponderous. It is, perhaps, on a ſimilar principle that we ought to account for the thick ſkins of the inhabitants of hot climates, particularly Negroes.

Convinced of the impropriety of retaining the head in a perpetual ſtate of perſpiration, a ſtate that renders the ſmalleſt expoſure, by uncovering, extremely hazardous, I have lately, although expoſed the greater part of the day in an unſhaded boat, left off wearing a hat, and ſubſtituted a ſmall *tamatà* of green ſilk to intercept the ſun from my eyes. It is, in my eſtimation, a conſiderable luxury to preſerve the head free of perſpiration; and from the want of a hat, I feel no inconvenience whatever. I have ſtill another, and perhaps more important purpoſe in view by leaving off a hat; which is, to demonſtrate to the world, that the diſeaſe fantaſtically termed Coup de Soleil, proceeds from very different cauſes than expoſure of the head to ſolar heat; — but of this in another place, and at another time.

Citizen Bruant obſerved in the courſe of his Egyptian practice, that Ophthalmia and Dyſentery often alternated with one another; and for their removal, he ſometimes applied bliſters to the calves of the legs. The fluids, ſublimed by heat, and ſometimes repreſſed from the ſurface by cold, evidently ſought for an outlet. To bliſters, therefore, I would have preferred ſetons, or iſſues, or occaſional phlebotomy. In inveterate Ophthalmia, nothing can equal the efficacy of a ſeton or iſſue at the nape of the neck; yet no *Franko-Egyptian* phyſician I have yet ſeen, ſays a word either of ſetons or iſſues.

Forms of Collyria.

No. 1. Of corroſive ſublimate, 6 grains; ardent ſpirit of any

any kind and pure water, of each 6 oz. Mix, and keep the eye constantly moistened with a cloth dipped in it.

No. 2. Of laudanum, a tea spoonful; ardent spirit of any kind, vinegar, water, of each 4 oz. Mix, &c.

To these two forms, particularly No. 2, I am inclined to give a decided preference.

No. 3. Of alum, 1 drachm; water, 8 oz.

No. 4, 5, and 6. Collyria similar to No. 3, may be made with an equal proportion of nitrated kali, (common nitre); of vitriolated zinc, (white vitriol); or of cerussa acetata, (sugar of lead.)

No. 7. Goulard's extract, a tea spoon full; vinegar, water, of each 4 oz. Mix, &c.

No. 8. Vinegar, pure water, equal parts. Mix, &c.

No. 9 and 10. When none of the preceding articles can be procured, salt, or even fresh cold water, may serve as an imperfect substitute.

D. WHYTE, M.D.

Bay of Aboukir, July 8, 1801.

On a new Method of treating the Effusion which collects under the Scull after Fractures of the Head. By J. DEVEZE, *Officer of Health, of the first class, in the French Armies.*

OF the different cases which require the operation of the trepan, I shall only consider the effusion between the dura mater and the scull, occasioned by blows and fractures.

Mr. Petit, a celebrated surgeon of Paris, has contributed greatly to the improvement of this art, by pointing out the particular symptoms which distinguish effusions under the scull from concussion of the brain. These different accidents equally result from falls or blows received on the head; and previous to this distinction it was easy to confound them, a mistake highly prejudicial to the patient who is affected with concussion only, as it requires a different treatment from effusion, and is not relieved by the trepan.

When there is a collection of blood from a blow or fracture of the scull, all authors advise the trepan, in order to discharge the collected fluid; but the difficulty of ascertaining the part where it has accumulated, often makes frequent repetitions of the operation necessary before it is discovered. Mr. Mareschal, first surgeon to Louis XIV. gives us an example of this; he trepanned

trepanned a young lady twelve times before he found the effusion occasioned by a fracture of the parietal and temporal bones on the same side. This case, and many others of a similar kind too numerous to relate, evidently show how important it is to render an operation more easy, which is often repeated without real necessity, is painful to the operator, and sometimes fatal to the patient.

I do not flatter myself with having obtained this object; but I think it a duty to communicate to the public some ideas which have occurred to me, and by which I have been so happy as to succeed, in a case I had an opportunity of attending in the French hospital established at Philadelphia.

In cases of accumulated blood between the scull and dura mater, the adhesion which unites them is destroyed in the place occupied by the fluid, the collected matter is circumscribed in a larger or smaller space, it expands the dura mater, and forming a tumor that oppresses the brain, produces the effects which require the operation of the trepan.

In this case a single opening made in the scull on one of the points of effusion, is sufficient to give vent to the fluid, because the blood, pressed on all sides by the action of the brain, quits the place it had collected in, and flows towards a part that offers a passage. It happens in this case, as it does in that where the accumulation exists between the dura and pia mater, that the blood, equally pressed by the brain, runs between those two membranes, flows towards the opening by the trepan, and presses the dura mater outward, which indicates to the operator that this membrane requires incision, in order to give passage to the collected fluid.

It is only in the first case that the adhesion of the dura mater to the cranium, by retaining the fluid, requires a repetition of the opening, should the operator not meet at first with the precise spot where the fluid is collected.

To avoid these inconveniencies, I propose in such a case to destroy the adhesion which unites the dura mater to the scull, and establish a communication between the collected blood and the opening already made by the trepan; by this means a repetition of trepanning would be avoided, and the operation becoming more easy might save the life of the patient in any case not necessarily mortal, which is particularly interesting when the effusion is situated at the bottom of the scull.

If instead of trepanning twelve times, Mr. Mareschal had separated the dura mater from the cranium, following the direction of the fracture, he would have certainly reached the effusion, and the blood would have been evacuated by the first opening, although it must have risen against it own weight; this

this will be easily understood by physiologists, who advert to the force of pressure the brain exercises on every part of the scull, and compare it with the resistance the collected blood may oppose by its specific weight.

The danger arising from a separation of the dura mater, may perhaps be considered as forbidding the method I recommend; but experience shews this separation is not dangerous, since, as I have already said, blood cannot collect between these two parts, without separation, and yet they return to their natural state, when the fluid is evacuated by the trepan, even where the separated parts have long remained divided from each other by the interposition of the fluid.

Of the cases which support my opinion, it will be sufficient to mention the following.

A young person, after the scarlet fever, had a violent pain which fixed itself at the upper part of the head; every thing art could indicate was tried to effect a cure, bleeding, bathing, cathartics, internal remedies, topicals of every kind, and blisters on the affected part, all had failed; when I was consulted, I advised the moxa, which was applied to the diseased part, and though a plentiful suppuration followed, the pain seemed to increase, and for six months continued to augment. When I was again requested to give my advice, I prescribed the trepan, which operation was immediately performed, in the centre of the painful part; the opening made in the scull by this means gave vent to a quantity of pus of a greenish white colour; the pain ceased entirely, the patient was soon cured; and since has enjoyed a perfect state of health.

The preceding observation clearly shews the dura mater had been long separated from the scull by the matter, and proves that the separation of this membrane is not dangerous.

It will be said, perhaps, that this separation did not produce any bad effect, because it took place gradually; my answer is, an effusion occasioned by violent blows is suddenly formed, it forces the dura mater from the cranium with violence, and separates it sometimes to a great extent. It may be again objected that nature, though acting hastily, manages in a manner art cannot imitate in separating the dura mater from the scull. I will oppose this objection by experience, and not by argument.

The 29th of March, 1795, there was brought to the French hospital established in this city, a man about thirty-eight years of age, of a middle size and very robust constitution: he was comatose, his face inflated and discoloured with ecchymosis, his body covered with bruises, and many wounds made with pointed instruments. Those who brought him, told me he had been

struck

ſtruck with an iron bar, which fractured his ſcull; and had been trepanned on the ſpot.

After uncovering the head, it was waſhed and ſhaved; and I found the trepan had been applied on the upper part of the right parietal bone, about an inch from the coronal ſuture.

I took away, with the lenticular knife, pieces of the internal plate which wounded the dura mater, and enlarged the wounds in the direction of the fracture on which the trepan had been applied; it proceeded from the ſagittal ſuture, and deſcended almoſt in a right line into the temporal region, at the upper part of which I bounded my inciſion, although the fracture extended lower; I obſerved another fracture in the upper and lower part of the ſame parietal, which had ſeparated a piece of the bone about three inches long, and two wide; this piece was neither indented nor diſplaced, was behind and a little above the part trepanned: blood iſſued from the ſuperior and poſterior fractures.

The next morning the patient was in the ſame ſtate, inſenſibly voided his urine, and could not ſwallow. The dreſſing was removed, much blood came from the opening by the trepan, and from the fractures.

When I viſited him in the evening he was comatoſe; but little blood came from the wound, nor did the preſſure I made on the dura mater produce more. I introduced a blunt flexible probe under the ſcull, in the direction of the fracture, from whence the blood proceeded in the laſt dreſſing, and endeavoured to do the ſame by the fracture which deſcended to the temporal region; at the diſtance of about a quarter of an inch I was ſtopped by a ſudden reſiſtance, and it was at that moment reflection ſuggeſted the method I immediately put in practice.

I preſumed the comatoſe drowſineſs which continued, was occaſioned by collected blood, and that it exiſted under one of the points of the fracture in the temporal region, becauſe thoſe effuſions which had been formed under the other fractures, were evacuated in the preceding dreſſing.

Had I followed the uſual method, it is poſſible I might have made many openings before I had ſucceeded, or have failed finding it; conſequently, after the reflection which ſuggeſted that the adheſion between the ſcull and the dura mater might be ſeparated without inconvenience, I determined to ſeparate the membrane by following the direction of the fracture; and proceeded to this operation with a ſilver ſpatula very flexible, the extremities of which were rounded: I took the precaution to preſs it towards the bone, and to bend my inſtrument by degrees as I entered, to make it take the form of the part upon

which I acted, and often drew it back, to meaſure on the outſide the way it had made. At length, after having entered half an inch below the temporal ſcaly ſuture, the reſiſtance ſuddenly ceaſed, and my inſtrument entered a hollow part, at the ſame moment the blood flowed in great abundance; when it ceaſed I drew out the ſpatula, which was followed by a ſmall quantity. The patient then began to move ſtrongly, tried to riſe and talk, without knowing what he ſaid.

The next morning I found him tied in his bed; this method was neceſſary, becauſe he endeavoured to riſe, as he ſaid, to go and fight. More blood came away at the dreſſing. In the afternoon I found him better; he drank plentifully, and anſwered my queſtions: the next morning, being the fourth after the accident, he had perfectly recovered his ſenſes, and from that time continued to mend. As his head had been much wounded, many abſceſſes were formed on the exterior; the laſt was on the piece of the parietal bone already ſpoken of, and as it had no connection, and was vacilating, I eaſily took it away, and the dura mater recovered and followed the motion of the brain: the wound had ſuppurated and the cicatrix was much advanced, when the patient went out the 28th of December.

During the cure, the patient felt no pain in that part of the head where I had ſeparated the dura mater; the cure of this trepan was neither longer nor more difficult than uſual, if we except the complication from the gatherings, which is foreign to the ſubject.

I cited Mr. Mareſchal's obſervation, becauſe the caſe is ſimilar to that which makes the ſubject of this eſſay. Mr. Mareſchal's patient had a fracture which croſſed the parietal and temporal bones: mine had fractures in the ſame bones, and ſame places; there was alſo another, and ſome very ſerious bruiſes, which made the diſeaſe complicated. Mr. Mareſchal trepanned his patient twelve times; I cured mine with one operation, and by a method which, to the beſt of my knowledge, had never before been tried.

To the Editors of the Medical and Physical Journal.

Gentlemen,

IN compliance with the requeſt of your correſpondent, Mr. Cuſtance, of Kidderminſter, I have ſent you a drawing of the inſtrument I took the liberty of recommending to the attention of

of the Profeffion, in a former number of your ufeful Journal. Had Mr. C. been induced to have given it a trial, I am confident he would not have required any farther information refpecting it, but have learned from experience "its mode of action," without which I cannot but confider his obfervations rather premature. Thofe gentlemen who are much in the habit of extracting teeth in London have given it the moft decided preference to any other invention; and from my own experience, I am convinced, very defervedly. The plan Mr. Whitford has adopted for the fale of his inftruments is a proof of their having given general fatisfaction; out of two hundred, which he has difpofed of within twelve months, not one has been returned, which was the condition of their fale.

I have alfo fent you a drawing of an inftrument made by Mr. Whitford, from my own inftructions, which, for the fake of diftinction, I have named, "Reece's Odontagra." I fhall briefly notice its peculiarities, and leave your readers to determine as to its merits. Should any of the purchafers of Mr. Whitford's inftrument be inclined to give it a preference, I am defired by Mr. W. to fay, that he will with pleafure exchange it. It differs from Mr. Whitford's in the curvature of the claw (A), the fulcrum (C) being longer, with a depreffion to receive the tooth, and with a fpring (F), to fix the claw. My principal objects to accomplifh in this invention, are, 1ft. To extract the tooth in as perpendicular a direction as poffible, which is nearly effected by the curvature of the claw (A), being more than a femicircle. 2dly. To prevent the breaking of the tooth, and facilitating its extraction, the end of the claw (B) is a little reflected, that it may be introduced between the fcarified gum and tooth, as far down as the alveolar procefs will admit of, and prefs on a greater portion of the tooth. 3dly. To avoid the great pain and injury of the gums from preffure by the ftructure of the fulcrum (C)* in the upper part, which is a concavity to receive the tooth oppofite the upper part of the reflected end of the claw, the preffure of the gum portion (E) does not fall on the contiguous foft parts till the tooth is a little elevated, which afterwards, for its complete expulfion, is very trifling. 4thly. To prevent the claw flipping in the operation by the fpring (F), and which, by taking firm hold of the tooth, fo as to be withdrawn, is fixed between the fulcrum and claw, renders any further ufe of the fcarificator and forceps unneceffary, which fo often alarm the patient with the idea of a fecond operation.

When

* Which rotates on either fide, on the fhank (G).

When the caries of a tooth is gone to that extent, as to leave only a bare ſhell on one ſide, or the whole of the corona deſtroyed, ſo that the remains are level with the gum, the poliſhed part of the fulcrum muſt of courſe reſt on the gum; but from the abſorption of the alveolar proceſs, and the looſe connection of the fangs with the ſocket from the ſame proceſs, the force required for their expulſion is attended with neither pain nor miſchief; and from the formation of the end of the claw, performed with greater eaſe than the uſual way of *punching*.

It is, I think, much to be lamented, that the operation of tooth-drawing ſhould be deemed by the Faculty of ſo little importance, as to be left to the practice of people both ignorant of its nature and ſtructure of the parts, notwithſtanding the frequent occurrences of the ill effects of the rude and unſcientific removal of teeth; beſides, I conceive it often a nice queſtion in ſurgery, to determine whether the extraction of the tooth be abſolutely neceſſary. A caſe ſome time ſince occurred in Monmouthſhire, of a lady who fell a ſacrifice to inflammation ariſing from the irritation of a tooth. Soon after ſhe found it painful, ſhe ſent for a ſurgeon to extract it, which, on account of the contiguous ſoft parts being much inflamed, he poſtponed till the next morning, recommending in the interim, proper means to be taken for its reſolution. In the courſe of the night it had unfortunately increaſed, which on the following morning rendered the operation ſtill more objectionable. On the third morning, the inflammation had extended to the œſophagus and neck, ſo as to prevent deglutition; and on the 5th, it had ſpread to the breaſts, which were amazingly tumefied. About the 9th, a very extenſive ſuppuration took place. About the 11th, it aſſumed a gangrenous appearance, which ſoon terminated in mortification, in ſpite of the efforts of her medical attendants, who are very deſervedly eſteemed gentlemen of great profeſſional judgment and ſkill.

Should the inſtrument I now recommend to the attention of my profeſſional brethren, meet with their ſupport from the reſult of practice, I ſhall be amply rewarded for my trouble by the honour of their approbation.

I have the honour to be, &c.

RICHARD REECE.

Henrietta Street, Covent Garden.

A Caſe

A Case of Tympanites, in an Infant, relieved by the Operation of the Paracentesis.

With Remarks on the Case; and a critical Analysis of the Sentiments of the principal Authors who have written on the Disease. To which is subjoined, an Account of the Operation of the Acupuncture, as practised by the Japanese in Diseases analogous to the Tympany. Communicated to the Editors by Mr. W. Coley, *Surgeon, in Bridgenorth.*

B. A male infant aged eight months, was brought to me on April 29, 1797. On inquiry and examination I found him to have a moſt prodigiouſly diſtended abdomen, the diſtenſion extending quite to the thorax, the whole of which appeared to be greatly protruded. The ſwelling was equal over every part; and the whole of the integuments, from the ſternum to the pubis, as well as laterally, ſo far as diſtenſion could go, was of that extreme fulneſs that a bladder, however tightly filled, could not be more tenſe, or leſs elaſtic, for it would not admit of the ſmalleſt compreſſion. A ſonorous, or ſounding noiſe, on tapping the part with the hands was very clearly perceptible; and I thought I diſcerned an obſcure fluctuation in the hypogaſtrium, immediately after a diſcharge of urine from the bladder, which fortunately happened to take place at this time. The limbs were preternaturally ſmall; and the weight of the child evidently leſs than it could poſſibly have been, had the ſwelling conſiſted of an accumulation of any watery fluid. Its countenance was ſhrivelled and ſickly; there were a pallor and coldneſs of the ſkin, dyſpnœa, a weak pulſe, and a general muſcular emaciation. With theſe ſymptoms I alſo found that it had lately nauſeated the breaſt, by which it had been hitherto almoſt entirely ſuſtained; that it was frequently pining, had but little ſleep, was apparently in much pain, and had for ſeveral days loſt the power of relieving itſelf by inteſtinal crepitation. He paſſed from one to three looſe natural ſtools daily, and had ſo done from the time of his birth; and the urine was evacuated in proper quantity. The girth of the body, carefully meaſured over the moſt protruding part of the abdomen, was twenty-four inches and a half.

The origin of this diſeaſe was coeval with the birth of the child, for at that period the abdomen was obſerved to be tenſe and fuller than natural. For ſome months the progreſs of the ſwelling was ſlow, but evidently increaſing and continuing tenſe. Until very lately he had often paſſed wind downwards from his bowels with great force, by which he was generally relieved.

No

No remedies were made uſe of, or any particular notice taken of this intumeſcence until a few weeks before I ſaw him; when, on account of his being in other reſpects ill, and the ſwelling beginning rapidly to increaſe, he was taken to a medical gentleman who gave him ſome purging pills; from which however no benefit was obtained.

I directed ʒjſs of the following ointment to be tenderly, but thoroughly rubbed into the abdomen every night; and one of the diuretic powders to be taken twice a day.

℞. *Ung. digital.* ʒvj. *Ung. hyd. fort. Sap. moll. a.* ʒjſs. *M.*

℞. *Fol. digit. pulv. gr. iij. Sacch. alb.* ℈j *m. f. pulv. xii.*

May 5. The child living in the country, I had not ſeen or heard from him until this day, when he was again brought to me. On the moſt ſuperficial view of his countenance, it was eaſy to diſcern that ſome material amendment had occurred. The eyes, that before were inirritable and hardly moved in the ſockets, were now clear, active, and noticing the different objects around; while his cheeks glowed with natural redneſs, and were plumped with increaſing fleſh. The appetite was voracious; his mother obſerving that it was as much as ſhe could do to ſupply him with food and the milk of her breaſts; and that he ate bread and butter greedily, though he had never taken any before. The ſwelling had decreaſed two inches, evidently the effect of a diuretic operation from the medicines. On examining the abdomen, which was done while he was held upright, it felt evidently leſs tenſe, more elaſtic, (becauſe admitting of reſiſtance) ſtill ſonorous, but with a fluctuation, as I thought, manifeſtly perceptible in the hypogaſtrium. The thorax as well as the abdomen appeared leſs diſtended.

On the ſecond night after taking and uſing the medicines, it was obſerved that he made an unuſual large quantity of urine; and from that time to the preſent he continued paſſing large quantities in the day, and wetting ſeveral cloths in the nights. His mother hardly recollects which occurred firſt, the increaſe of appetite or the *diureſis*; but ſhe believes both about the ſame time.

The medicines were ordered to be diſcontinued; and a cordial mixture was preſcribed to be given three or four times a day, with a ſuitable diet, excluſive of the uſual lactation.

May 12. The child continued to paſs an increaſed quantity of urine only for a day or two after I laſt ſaw him; ſince which time the ſwelling has returned to within half an inch of the girth it meaſured on April 29; yet the appetite remains good, and his health in other reſpects is apparently improving.

The powders and ointment to be repeated as at firſt.

May

May 17. The ſwelling now meaſures half an inch more than when I firſt ſaw him. From the 12th to the 14th his mother inadvertently omitted giving him any medicine. He is now ſickly, has no appetite, nor any increaſe of urine; the breathing more troubleſome than ever.

Medicines to be continued.

May 22. The intumeſcence continues, but is not increaſed. Appetite ſtill bad; loſs of ſtrength and emaciation faſt returning.

The medicines to be yet continued.

June 5. No alteration of the ſwelling, but he gets weaker; draws but little milk from the breaſt, and takes no other food; waſtes in his fleſh, and the countenance is become pale and ſickly, with great torpor and inirritability of the whole ſyſtem. No ſenſible alteration from the medicines.

Rep. pulv. cum duplo digit. et utatur ung. ut ante.

10th. Symptoms ſtationary.

The powders to be repeated, increaſing the fox-glove to ten grains for twelve doſes; and the ointment to be continued.

17th. Every ſymptom the ſame as when I firſt ſaw him, except the ſwelling being ſomewhat larger. Evident danger of the integuments rupturing from their exceſſive diſtenſion, and not allowing of the ſmalleſt compreſſion. No probability, without ſome ſpeedy and effectual relief, that he can live many days.

The medicines failing to produce any ſenſible operation, were now ordered to be omitted; and the *paracentesis abdominis* was recommended. But the parents of the child objecting to have an operation performed, it was at preſent omitted; nor would they conſent to have any other remedy uſed, imagining it was impoſſible the child could live long, and that no operation or medicine would be likely to be of any uſe. Yet on the 21ſt in the evening, the father of the child came to requeſt I would perform the operation as a laſt reſource, becauſe I had ſaid that it might ſave the child's life; and he now thought that without immediate relief it could not poſſibly ſurvive in its preſent condition many hours. An unavoidable engagement prevented my attendance until ten o'clock the next morning, (June 22) when the ſtate of the child was ſuch as left me but little room to ſuppoſe he could live one hour; indeed, it had much the appearance of being then actually dying; for, together with a coldneſs of the whole ſurface, manifeſtly greater on the extreme parts of the body (though laying on its mother's lap in a cloſe room near to a fire) and a very weak ſlow pulſe, his eyes were fixed and half cloſed; and frothy mucus was, and had been for ſome hours, conſtantly iſſuing from his mouth

and nose; the breathing short, tight, and intermitting; his countenance the most complete *facies hippocratica* I ever witnessed. He had not eaten any solid food for some days, entirely subsisting on lactation; nor of this could he take much, as the obtaining of it extremely distressed him, from the great difficulty with which he breathed.

The *paracentesis* with a small trocar was performed, by thrusting the instrument about one inch into the abdomen, on the left side a little below the *umbilicus*; which was done while the child was laying on the lap of its mother. On withdrawing the perforator, there forcibly burst from the canula a vast quantity of air, (I should think not less, and probably more than two gallons) with a hissing noise that was distinctly heard at many yards distance; immediately followed by a flow, at first in a full stream, afterwards more gradually, of a very foetid liquid, exactly similar to a *liquid fæcal stool*, amounting in the whole to about a pint and a half, and not varying in its appearance from the first to the last drop. In this liquid were a vast many lumps of undissolved *fæces* of various sizes, some smaller than a split pea, and some as large as could possibly pass through the canula, the internal diameter of which was exactly one sixth of an inch. A subsidence of the abdomen on the flowing out of the air was instantly visible; and in less than two minutes there was a most complete collapse, not any of the swelling remaining. A plaster with lint was put to the wound, (from which not a drop of blood issued) and a flannel rather moistened with spirits applied round the body. After the child had recovered the fright, and doubtless some pain occasioned by the puncture, which he did in one minute, it was a pleasing sight to see him turn and as it were solicit the breast of his mother, which he seized eagerly, and seemed to enjoy with peculiar satisfaction; at one moment leaving the nipple to look around, and then returning to it with increased pleasure, which he repeatedly did for a considerable time. Indeed his breathing and altered countenance *(vita quam a morte restaurata)* evidently shewed the ease and comfort he now enjoyed. He also swallowed a little warmed wine and water; and this was ordered to be repeated during the day, as a cordial, and to obviate fainting.

June 23. He has appeared to be very comfortable ever since the operation; breathes easier, is warmer, and in many respects better; but is extremely weak, and no part of the countenance except the eyes appear mended. He takes the breast freely. This evening the roller being somewhat loose, it was re-applied by his mother, who thought there was somewhat more swelling than when it was first put on. Ordered the following medicine.

R. Tinct.

℞. *Tinct. cinchon.* ʒvj. *Sp. æth. vitr.* ʒj. *Syr. sim.* ℥ij. *Conf. arom.* ʒj. *Fol. digit. pulv. gr.* ij. *m. detur cochl. min. ter die.*

24th. Continues nearly in the same situation as yesterday, except that the abdomen is becoming hard and evidently increasing in size. Makes water freely; but has not had any stool since the morning of the 22d, before he was tapped.

Ordered to persist with the mixture; and to have in the evening a glyster of sugared milk and oil, if no stool should be discharged before that time.

26th. He had a natural stool, therefore the glyster was omitted. He is now exceedingly improved in his health, takes the breast frequently and eats food very often, makes urine in full quantity, is playful and apparently easy, though the abdomen is again swelled as high as the *scrobiculus cordis*, and measures twenty-four inches and a half; tense, elastic, and sonorous, when tapped with the hands. Has not had any stool since the evening of the 24th.

Cap. illico jalap. gr. iv. Calom. gr. j. pro cathartico; dein persistat usu et ante mist. et ung. repet.

27th. He had two or three stools from the powder. Girth of the swelling to day twenty-five inches, in other respects full as well as yesterday; eats heartily, &c.

July 1. The swelling continues stationary, has natural stools and they are of a thicker consistence than usual; voids much wind by the anus, makes water plentifully, appetite good.

The following was now prescribed to strengthen the relaxed bowels, and to enable them to expel the air more freely.

℞. *Ext. cinchon.* ℈iv. *solv. ex. Aq. calid.* ℥ij. *Tinct. cinch.* ℥iss. *Tinct. fer. mur. gt.* 80, *Syr. tolu.* ℥ivss. *m. Detur.* ℥ss. *ter die.*

9th. The swelling is still twenty-four inches and a half over the abdomen, but it does not increase upwards; continues in other respects healthy; evacuates wind largely *per anum.*

Cap. calom. gr. j. mane quartis diebus pro cathartico; et persistat mistura intermediis diebus.

17th. The calomel gave him three or four stools, and the fulness appeared afterwards to be somewhat diminished, but is now of the same girth as on the 9th, and appears to be extended more upwards. The whole of the swelling seems to be palpably flatulent. In other respects there is no change.

I now wished the paracentesis to be repeated, but his friends would not consent to it. The tonic mixture was therefore ordered to be continued, a carminative cathartic to be taken every fourth day, and the body to be immersed in cold water every morning, except on the days of taking the purges.

 Aug.

Aug. 5th. He has been weaned the laſt four or five days; continues hearty, but with little alteration in the ſwelling. Has of late ſometimes had ſpontaneous *diarrhœa*, on conſideration of which the purges have not been adminiſtered; nor has he been bathed more than twice, on account of the trouble it gave to the aſſiſtants, and the fright it occaſioned to himſelf.

He was ordered to take ten drops of muriated tincture of iron three times a day.

Sequel.

What quantity he took of the muriated iron is uncertain, but I believe it was not much; nor after this time was any medicine given to him, except a purge occaſionally on account of obſtipation. In this interval another mode of evacuating the air, which was by introducing a glyſter-pipe into the rectum, was tried a few times on the ſuggeſtion of Dr. Darwin, (*Zoonom. vol.* 2); but it did not from any of the trials appear to have been uſeful, and was certainly leſs ſo than the exhibition of a common glyſter; and this, indeed, was generally neceſſary before the introduction of the pipe, as otherwiſe the end of it became ſtrongly clogged with hardened fæces. His friends would not conſent, as has been before ſtated, to have the paracenteſis repeated; ſo that it remains unknown what might have been the conſequence of reiterated perforations as a temporary or laſting relief. The diſeaſe, at length, occaſioned the death of the patient though he lived nearly eighteen months from the time of the operation. It ſomehow unfortunately happened that I did not hear of the fatal event until it was too late to examine the dead body; a circumſtance that I much regret, as diſſection (if it could have been obtained) might have clearly elucidated the real nature of the diſeaſe.

Remarks on the above Caſe.

The mode of treatment adopted in the preceding caſe, I think, clearly proves that ſome portion of water was contained in the abdomen along with the air; and that the mercury and fox-glove were the agents by which that fluid was expelled.

It may be inquired, ſince the intumeſcence was ſo conſiderably leſſened on the 5th of May, why I directed the medicines to be diſcontinued, and another plan to be proſecuted? Why there ſhould be any alteration propoſed, while the ſwelling was ſo kindly ſubſiding? To ſuch queſtions I can only reply by explaining the circumſtances that influenced my opinion at the time, which were, that from the very conſiderable diuretic operation of the medicines I had ſtrong reaſon to ſuſpect ſubſequent debility,

debility, with nausea, and other effects that are well known to follow the exhibition of the diuretic I had made choice of, which symptoms I wished to avoid; and from the state of the abdomen, I was well satisfied that the water it contained was in trifling quantity; and consequently, that the remaining swelling was chiefly, or entirely, flatulent. But to put this point entirely out of doubt, the *digitalis* was afterwards repeated, and its dose increased, until it appeared beyond all suspicion that its further exhibition would be useless.

The imminent danger in which the child appeared at the time the operation was performed, was such as to leave scarcely any hope that he could have survived it; yet, it is most evident that without such relief as the perforation effected, he must inevitably have died in a very short time.

I find, however, after a pretty extensive search for the purpose, that there are but very few instances of the tympanites recorded (I have not met with any distinctly related) which have been treated by the paracentesis, or puncture of the abdomen. But as in this case the operation was attended with such happy effect at the time, it is much to be regretted that I had not permission to repeat it as occasions might have required; by which, probably, the child's life might at least have been prolonged, though the disease might not have been cured.

The fæcal discharge that followed the puncture must have come from the bowels; but whether from perforation made by the instrument in the operation, or from some preternatural or originally formed aperture in the bowels coexistent with the formation of the child, or from an intestinal orifice any way occasioned by worms, or by disease, I cannot pretend to determine; and whether therefore this disease were originally and exclusively an abdominal, or an intestinal case, perhaps cannot be very certainly pronounced; we only know, that whatever it were before the operation, from that time it must have been of the latter kind, or a complication of each, consisting of air collected both within the bowels, and also in the peritonæal cavity. I think it not improbable, but in the original conformation of the intestines, some opening or aperture might have existed; or worms in the intestines might have produced such a perforation as would cause the communication which probably occurred in the early part of the disease, or inflammation of the bowels, ending in a very limited sphacelation of a part of them, might and perhaps would occasion necessarily a tympanitic affection. For it is difficult to conceive, how air can be generated in the cavity of the peritonæum in such prodigious quantity without some communication with the intestines; and it is very properly questioned by a writer of sterling eminence, (Dr. Cullen) whose

whoſe opinion I ſhall quote below, whether ſuch a caſe as the *T. abdominalis*, ever does exiſt without ſuch communication?

The other remedies made uſe of, as they were of no material ſignificance, it is unneceſſary to comment upon.

A Critical Analyſis of the Opinions of the principal Authors who have written on Tympanites.

Moſt of the writers whoſe ſentiments have been detailed on the ſubject of tympany, conſider it as a diſeaſe of very rare occurrence, under whatever form it may appear; and there are ſome, it is to be ſuſpected, as will be evident from the following analyſis, who have detailed their opinions, while they have not really ever witneſſed any caſe in ſupport of them.

Celsus, a phyſician or ſurgeon, who practiſed early in the firſt century, has appropriated a part of one of his chapters "*de medicina*," to the cure of this diſeaſe. Indeed, his practical directions are moſt excellent, and clearly ſhew that he had ſeen the diſorder he treats of. Vomiting, warm and dry fomentations, cupping and ſcarifying, and, as a laſt reſource, *per alvum infundere aquam alidam*, are the remedies he recommends. He alſo adviſes vehement rubbing of the part three or four times a day with oil impregnated with ſome warm ſubſtance, ſuch as muſtard, *donec cutim erodat*; and the actual cautery to be applied to ſeveral parts of the belly, to produce exulceration; the ulcers to be kept a long time open. Boiled ſquill, he ſays, is alſo uſeful to be bound upon the ſkin. Laſtly, he enjoins for a long time after the diſeaſe, *(poſthac inflationes)* that the ſick abſtain from every thing of a flatulent nature. (*Celſus*, *lib.* 3, *cap.* 21.)

Ambrose Parey, whoſe works were made public more than two hundred years ago, (his epiſtle dedicatory bears date Feb. 1579) in the 11th chapter of his 8th book, treats of the dropſy. He deſcribes the Tympanites, and what he calls the moiſt *aſcites*; alſo particular dropſies, ſuch as the *hydrocephalus*, *pleurocele*, *hydrocele*, &c. But it is remarkable, that notwithſtanding he is very particular in his mode of curing the dropſy, in which he recommends bags, baths, liniments, emplaſters, &c. and takes great pains to refute the miſtaken opinion of Eristratus, who condemned the paracenteſis as deadly; yet he does not give one word of direction reſpecting the treatment of the tympany; from which it may be very reaſonably inferred, although his practice had been very extenſive, having, as he ſays, enjoyed the office of principal chirurgeon under three kings, that he had really never ſeen the diſeaſe.

Van Swieten, in his Commentaries upon Boerhaave's Aphoriſms, has collected much information from various authors reſpecting this diſeaſe. From the valuable and critical account

account which he has given, there can be no doubt of the exiſtence of it, in the two forms which modern Noſologiſts have deſcribed. He has adduced inſtances of both; but one of the caſes (from the Med. Eſſays, Vol. I.) was evidently an inſtance of the *T. inteſtinalis*, which is probably the only ſpecies that is curable. The caſe which he mentions from *Helmont*, in which the ſubſidence of the abdomen was inſtantaneous after the operation of the paracenteſis, proved almoſt ſuddenly fatal from the operation; but why fatal is not accounted for. May it not have been from arterial hæmorrhage, as in wounding the epigaſtric artery with the inſtrument? Some inſtances of which we know to have happened, and are recorded in Medical Communications, Vol. II. This patient of Helmont's, it ſhould be noted, was tapped for an aſcites, nor was it known till after the operation what was the nature of the caſe.—Van Swieten's opinions of the paracenteſis in the tympany is, that the attempt would be hazardous; though he admits that where certain deſtruction is at hand, it is juſtifiable to uſe a doubtful remedy. He judiciouſly recommends that the trocar ſhould be of a ſmall ſize, that if a wound be inflicted in the inteſtine it may more certainly and ſpeedily cloſe, than if a large inſtrument were employed. In one of his obſervations he is unqueſtionably wrong; it is this, "In a tympany the abdomen is never diſtended to ſo vaſt a ſize as in an aſcites; but is flatter and more compreſſed towards the ſides, and more prominent before." (*Edin. edit. tranſlated from the Latin, vol.* xii, *ſect.* 1226.) In the caſe above recorded, and in the only one other that I ever ſaw, the diſtenſion of the whole of the integuments of the abdomen was as great as could poſſibly be, and certainly in every part more tenſe than in any inſtance of aſcites.

HOFFMAN ſays, this is a diſtemper that rarely happens, and is of opinion that it ſhould rather be pronounced a ſymptom of the *anasarca* and *ascites*, than denominated a certain ſpecies of the dropſy. He recommends for the cure of it carminative glyſters; and affirms, that purging pills joined with an opiate have done great ſervice in this caſe; he alſo adviſes the whole of the abdomen to be anointed with camphorated oil for ſome days running, which gives ſome relief. (*Swan's edit. of Sydenham, 4th edit. p.* 514)

JUNCKER's opinion is, that powerful evacuations are not ſo uſeful in this diſeaſe as in the aſcites; but gentle evacuations by ſtool, urine and ſweat are beneficial. (*Ibid.*)

HEISTER ſays, in a tympany which is owing to watery humours, the relaxed tone of the viſcera, and wind, which greatly diſtend the flaccid bowels and abdomen, ſtrengthening and carminative medicines ſhould be mixed with reſolvents,

bitters

bitters and ſaline medicines, or both given alternately. He alſo recommends purging, a proper diet, and warm liniments or plaſters for the abdomen. (*Ibid.*) The *T. abdominalis*, he aſſerts, is a caſe very rarely found, though an inſtance of it quoted by him from RUYSCH, who performed the diſſection in the preſence of Heiſter, clearly evinces its exiſtence. Heiſter adds, "Dr. R. though a very old anatomiſt, had never ſeen an inſtance of it before; and I, who am now ſeventy years of age, and have ſince that time, opened the bodies of many perſons ſuppoſed to have had a true tympanites, never afterwards met with any air in the cavity of the abdomen." (*Wirgman's translation of Heister's Med. Chir. et Anat. Cases, 4to p.* 21.)

Dr. MEAD, though ſhort in his account of this diſeaſe, is very explicit. He diſtinguiſhes two kinds of tympany: one he calls abdominal, which is, when the abdomen is bloated up with a vapour, cauſed by exhalation from ſome mortified viſcus, of which he gives a notable inſtance; ſecond, the tympany which happens without any putrefaction, from an elaſtic air engendered in the inteſtines, which it ſtretches to ſuch a pitch as to deſtroy their contractile power, &c. The firſt ſpecies, which proceeds from mortification, he believes to be abſolutely incurable. The other ſpecies, he ſays, is to be treated with moderate cathartics frequently adminiſtered, and carminatives; with diet of very eaſy digeſtion. He alſo recommends exerciſe, and large glyſters of warm water, and cauterizing the abdomen as preſcribed by Celſus; or, in place of uſing the red hot irons, to lay bliſters on the abdomen. (*Mead's Medical Precepts and Cautions, ch. 8th.*)

Sir JOHN PRINGLE (*Diſeases of the Army*, 4to. edit. 5th. p. 214) had inſtances of this diſeaſe, though, he ſays, they "did not often occur;" he attributed them to "the unſeaſonable uſe of opiates in the dyſentery, or of the bark in intermittents." The caſes he ſaw were diſtenſions of the colon from air, and they yielded to rhabarbarine cathartics, aromatics, and tonics. "All ſtrong phyſic, and carminatives without laxatives, were hurtful. If any fever was preſent he began the cure with bleeding, and gave the common ſaline mixture with rhubarb. From the event of one caſe, which ended ſuddenly fatal, owing to the belly ſubſiding all at once, the conſequence of three or four looſe ſtools, he adviſes the patient to be ſwathed, as thereby a compreſſion can always be made ſuitable to the decreaſe of the air in the bowels.

CULLEN, the late learned Profeſſor of the Practice of Phyſic at Edinburgh, has given a more full, accurate, and ſcientific hiſtory of this diſeaſe than any perſon before his time. He deſcribes, in common with the moſt correct of the Noſological writers,

writers, two ſpecies of the tympanites. 1. *T. inteſtinalis.* 2. *T. abdominalis.* His idea of the proximate cauſe of the diſeaſe is, that it conſiſts in "a loſs of tone in the muſcular fibres of the inteſtines," and that the interruption of the paſſage of the air therefrom is dependant upon "ſpaſmodic conſtrictions in certain parts of the canal." This firſt ſpecies of the diſeaſe is the only one, in his opinion, that can be cured. The other ſpecies, he thinks, is not a primary diſeaſe; is an affection of very rare occurrence, if indeed, as is very doubtful, it ever exiſts ſeparately from the *inteſtinalis*; is not eaſily to be diſtinguiſhed; and when known, does not admit of a cure. Indeed, his opinion of the firſt ſpecies is, that it is generally a fatal diſeaſe, ſeldom curable, and hardly ever "but in a recent caſe." For evacuating the air accumulated in the inteſtines he adviſes gentle laxatives only," as ſtrong ones, "in the tenſe and over-diſtended ſtate of the inteſtines, would endanger the bringing on of inflammation." Glyſters alſo, when the fæces are in a "hard and dry ſtate," become neceſſary; and particularly when they produce "a conſiderable evacuation of air; as they muſt thereby have ſome effect in relaxing the ſpaſms of the inteſtines, they ought to be frequently repeated." For taking off the conſtrictions of the inteſtines, antiſpaſmodics of the milder kinds are recommended, and particularly "opium after the operation of a purgative." Fomentations and *tepid* bathing, long continued, have alſo been found uſeful for the ſame indication. To reſtore the tone of the relaxed bowels, tonic remedies, ſuch as "chalybeates, bitters, or the Peruvian bark, he thinks, are properly indicated;" and as there is "no tonic remedy more powerful than cold applied outwardly, and cold drink taken into the ſtomach," ſo theſe have been conſtantly preſcribed, and cold bathing has been advantageouſly uſed; but the moſt efficacious application of cold ſeems to be that of applying "ſnow to the lower belly, from which ſeveral inſtances have occurred of the diſeaſe being ſuddenly and entirely cured."* "Foſil acids and neutral ſalts, as antizymics, may be uſeful. As to the *paracenteſis*, "in obſtinate and deſperate caſes, he ſays, it has been propoſed, but it is a very doubtful remedy, and there is *hardly any teſtimony of its having been practiſed with ſucceſs.*" He believes "this operation is a remedy ſuited eſpecially, and almoſt only, to the *T. abdominalis*, which, however, is not very likely to be cured by this remedy; and how far the operation may be *ſafe* in the *T.*

* Quere, Where are theſe Caſes recorded?

intestinalis, he thinks, has not been determined by any proper experience." *(Cullen's First Lines, vol. 4.)*

Mr. BENJAMIN BELL, in his late System of Surgery, has communicated some valuable pathological information on this subject. Though that species of the disease which has the air confined in the intestines only is the most common, and which, he supposes, proceeds from the bowels losing their tone entirely; yet he speaks of one instance of the other species (or rather a mixture of the two) which fell under his own inspection, and mentions another that he had heard of; in both of which the air was found to have escaped from the intestines by a "*very small hole*" which was discovered in one of them.* He therefore thinks that the *T. abdominalis* very rarely proceeds from any other cause; and if it does, that no remedy will ever be able to effect a cure. However, from whatever cause the disease may have originated, and whether the air be contained in the bowels themselves, or diffused in the cavity of the peritonæum; so soon as it is found to be productive of much distress, he thinks no doubt should occur of the propriety of discharging it. Making a perforation into the abdomen for discharging air collected in the intestines, he observes, is no doubt a very formidable operation, and ought not to be attempted but in cases of real necessity; but as death has often ensued from this disease, he is clearly of opinion, when all the usual remedies have failed, that the operation ought to be performed rather than the patient should be consigned to die in certain misery. He recommends using a trocar of the smallest size; but it does not appear that he ever performed the operation in this disease. *(Bell's Surgery, vol. 2.)*

Dr. DARWIN (*Zoonomia, vol. 2, p.* 131.) also distinguishes two varieties of this disease. He says, that the air in the cavity of the abdomen is sometimes in a few days exchanged for water, and the tympany becomes an ascites.——Exclusive of some other remedies of usual recommendation, he advises "the introduction of a glyster-pipe into the rectum, to take off the resistance of the sphincter, and thus discharge the air." Improving on this idea, it has occurred to me that it would be practicable in this disease, having first emptied of their fæcal contents

* It would have been more satisfactory if Mr. B. had mentioned in what part of the intestines the holes were discovered, and whether in the same part in both patients. It may fairly be presumed that there were no worms in the bowels in these cases, or within the cavity of the peritonæum, otherwise such an occurrnce could not fail to have been mentioned. Van Swieten (*Com. on Boerhaave, Sect.* 1226) has observed, if worms have bored through the intestines, an abdominal tympanites may take place.

contents the rectum and such part of the colon as an injection would reach, to *pump out air from the intestines with some mechanical contrivance*, such, for instance, as was a few years ago invented by Mr. *Trye*, of Gloucester, to extract urine from the bladder.* I confess, the idea did not occur to me at the proper time, or I think I should have tried the operation of this ingenious contrivance. The introduction of a pipe or tube formed of a sufficient length and diameter, would hardly give more inconvenience to the patient, or trouble to the operator, than the passing up of a common glyster-pipe. The elastic gum or *caoutchouc* is a substance admirably well adapted for the purpose; and it might be made of such a length as to be introduced, I should conceive, without any hazard, through the whole length of the rectum into the colon itself; by which its power of extracting the air would be infinitely increased. It should be well oiled at the time of its introduction; and if any risk should arise of its collapsing from the warmth of the part, it might be conveniently kept permeable by having a spirally formed wire contained within it.

Dr. BAILIE, who is the last writer I shall mention on the subject, imagines it is "very probable that in cases of Tympanites the air is secreted by the small vessels opening upon the villi of the internal membrane of the intestines, and thrown into their cavity." (*Med. Chir. Trans. vol.* 1, *p.* 207.) Of what kind these air-vessels are I have no adequate conception; but can readily suppose, that if there exist any vessels within the intestines, or opening upon their internal surface, capable of such an operation as generating and expelling air, that the same also may exist in the peritonæal cavity, and thus readily produce a tympanites. They may, indeed, as well be supposed to have existence in various other situations or cavities of the body, and to give occasion to flatulent diseases in those parts. But as the former proposition is, I believe, without proof, so the latter must be considered (unfortunately for theory) as an assertion entirely gratuitous.

Of Acupuncturation.

Quere.—In the peritonæal Tympany could occasional benefit be derived by acupuncturation of the abdomen? Being a more simple operation than the paracentesis, it would of course have preference, at least with timid patients; but it is from practice only that we can learn whether it would be equally useful. This operation, I believe, never was practised much, if

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* See Trye's Remarks on morbid Retentions of Urine, 8vo. 1785.

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at all, in this country, as it has been in ſome parts of America;* and by the Chineſe and Japaneſe it has always been an operation of practice, in a great number of diſorders to which thoſe people are liable, but particularly in a diſeaſe very analagous to the tympany. It is by the operation of the acupuncture that they cure a ſpecies of cholic called *Senki*, which is ſaid to proceed from a rarefaction of ſome pernicious vapour in the cavity of the abdomen. Heiſter (*General Syſtem of Surgery, vol.* 1, *part* 2, *ch.* 18.) very ſuperficially mentions this operation. He has given drawings of two inſtruments which the Japaneſe make uſe of in performing it; but obſerves, that the practice has not been followed by any of our European nations; and therefore directs his readers for further information to conſult the writings of RHYN and KEMPFER, two ſurgeons who had been ſpectators of the operation in that country. But the beſt account I have ſeen of the acupuncture occurs in the ninth vol. of "*The modern part of an Univerſal Hiſtory, from the earlieſt account of times, 8vo.*" As the frequency of the operation demonſtrates at leaſt its ſafety, it may yet perhaps be thought by the Engliſh Surgeons on ſome occaſions to be worth imitating; and as the method of doing it is both curious and but little known, the following detail of it, from the volume mentioned, is tranſcribed for more public information, and to conclude the ſubject: "The place made choice of for the puncture is commonly at a middle diſtance between the navel and the pit of the ſtomach; but often as much nearer to or farther from either, as the operator, after a due ſcrutiny, thinks moſt proper; and in this, and the judging rightly how deep the needle muſt be thruſt below the ſkin, ſo as to reach the ſeat of the morbific matter, and giving it a proper vent, conſiſts the main ſkill of the artiſt, and the ſucceſs of the operation is ſaid to depend. Each row hath its particular name, which carries with it a kind of direction with regard to the depth of each puncture, and the diſtance of the holes from each other; which laſt ſeldom exceeds half an inch in grown perſons in the perpendicular rows, though ſomething more in thoſe which are made acroſs the body, thus,

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"The needles which perform the operation are made, as was hinted at firſt, either of the fineſt gold or ſilver, and without the leaſt droſs or alloy. They muſt be exquiſitely ſlender, finely poliſhed, and carry a curious point, and with ſome degree

* See Dampier's Voyages.

gree of hardneſs, which is given them by the maker by tempering, and not by any mixture, in order to facilitate their entrance, and penetrating the ſkin. But, though the country abounds with expert artiſts, able to make them in the higheſt perfection, yet none are allowed to vend them but ſuch as are licenſed by the Emperor.

"These needles are of two ſorts with reſpect to their ſtructure, as well as materials; the one, either of gold or ſilver indifferently, and about four inches long, very ſlender, and ending in a ſharp point, and have at the other end a ſmall twiſted handle, which ſerves to turn them round with the extremity of the middle finger and thumb, in order to ſink them into the fleſh with greater eaſe and ſafety; the other ſort is chiefly of ſilver, and much like the firſt in length and ſhape, but exceedingly ſmall towards the point, with a ſhort thick handle, chanelled for the ſame end of turning them about, and to prevent their going in too deep; and, for the ſame reaſon, ſome of them are caſed in a kind of copper tube, of the bigneſs of a gooſe-quill, which ſerves as a ſort of gauge, and lets the point in juſt ſo far as the operator hath determined it. The beſt ſort of needles are careſully kept in a caſe made of bull's horn, lined with ſome ſoft downy ſtuff. This caſe is ſhaped ſomewhat like a hammer, having on the ſtriking ſide a piece of lead, to give it a ſufficient weight, and on the outſide a compreſſed round piece of leather, to prevent a recoil, and with this they ſtrike the needle through the thickneſs of the ſkin; after which, they keep turning the handle about with the hand till it is ſunk to the depth they deſign it, that is, till it is thought to have reachad the ſeat of the morbific virus, which, in grown perſons, is ſeldom leſs than half, or more than a whole inch. This done, he draws it out, and compreſſes the part, in order to force the morbific vapour or ſpirit out. The directions and nice rules for the performing of this curious operation are many, and require great ſkill and attention in the operator; and, when duly performed, may be of excellent uſe not only againſt the excruciating diſtemper called Senki, but againſt many other topical ones, which are more commonly cured by the Indian *Moxa*, and other cauſtics. On the other hand, theſe laſt are often tried againſt the diſtemper above-mentioned, by applying the cauſtic to the belly, on each ſide of the navel, and about two inches from it, but moſtly without any ſucceſs, it being very unlikely that ſuch an application ſhould reach the ſeat of the diſtemper; whereas the benefit which hath accrued from the acupuncture, in that one diſeaſe, hath encouraged others to apply it indifferently to other parts of the body, where the *Moxa* is uſed; and, by a due care and precaution not to prick any nerves,

nerves, tendons, or other confiderable blood-veffels, have cured their patients by it, without putting them to the excruciating torture which attends that of the *Moxa*, or other cauftics.

Chemical Examination of Mr. SMITH'S *Flexible Metallic Bougies.*

To the Editors of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

I Had totally forgot that you announced the flexible Metallic Bougies in the fecond volume of the Medical and Phyfical Journal, but I have read lately fome account of their effects, and I was led to believe that fome confiderable improvement had been made in the treatment of a direful difeafe.

I am well aware that this is merely a revival of an ancient practice, and that Metallic Bougies have long been ufed in Surgery; abundant proofs of that fact may be found in the works of the authors who have treated on the fubject. I know alfo that the bad confequences arifing from the frequently repeated introduction of fo hard a body in the urethra, the membrane of which is fo delicate, and perhaps the difficulty of adapting to the urinary paffage a fubftance which, however foft and pliable, bends always at a more or lefs confiderable angle, had induced furgeons to have recourfe to the much milder means recommended by Daran, Goulard, and others.

Conceiving, notwithftanding, that every method of treating fo melancholy a difeafe as ftrictures in the urethra ought to be candidly examined, I procured fome of thefe Bougies, and I determined to fend fome to my valuable friend Francis Charoy, Surgeon in Chief to the eighth regiment of Huffards, garrifoned at Haguenau, in the department of the Lower Rhine. But before fending them, I thought it might not be altogether ufelefs to enquire into their compofition. A powerful incentive with me was, my being a decided enemy to any fecret in medicine, perfuaded as I am, that fecrefy is too often the cloak of empiricifm. If a remedy is fanctioned by repeated trials, a humane and generous practitioner will make it as public as poffible, that every fuffering individual may be benefited by it; if it has been propofed upon a fanciful hypothefis, as is often the cafe, even in this enlightened age, let it be rejected or admitted,

mited, as experience will direct after a fair trial. Had the immortal Jenner kept his admirable discovery a secret, mankind would have continued to fall victims to that dreadful scourge, the Small-pox.

In order, then, to contribute my share in the publicity of what I conceive to be an improved mode of treating strictures, I instituted some experiments which I shall here relate, as well as the inductions to which they conducted me. I will not stop here to describe the external appearance of these bougies, as they have nothing peculiarly remarkable, and as I suppose that most medical gentlemen are acquainted with them. They are smooth, firm, and possess a great degree of pliability. Those which I examined were of the largest dimensions (No. 11 and 12), they were nearly as flexible as common bougies of the same size. They were sold to me under the title of *Solid flexible Metallic Bougies.* Upon cutting them across, they were found *hollow*, their internal surface was very bright, and the ends were skilfully closed. I must here confess, that I am not sufficiently versed in the English language to determine whether hollow tubes, stopped at both ends, may with propriety be called solid bodies.

Experiments with Nitric Acid.

1. One hundred grains were put in a Florence flask with concentrate nitric acid, upon a sand bath; an abundant disengagement of nitrous gas took place, and a white matter remained at the bottom. This powder was carefully separated by filtration from the supernatant liquor.

2. Sulphuric acid and sulphates were dropped in some of the liquor. No precipitate of any kind was perceived. Hence no lead was present.

3. Water impregnated with sulphurated hydrogen gas caused no precipitate. This excluded at once silver, mercury, copper, lead, tin, antimony, bismuth, and arsenic.*

4. Carbonate of soda, dropped in the liquor, caused no precipitate; therefore no metal was present.

5. Gold and platina I did not expect to find in the white precipitate, and the following experiments shewed they ought not to be looked for.

Experiments

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* I prefer the words, *water impregnated with sulphurated hydrogen gas* to *hydrosulphurated water*, or *water impregnated with sulphurated hydrogen gas*, for reasons which will soon be published in "A Dissertation upon Chemical Nomenclature, by Rd. Chenevix, Esq. F. R. S. M. R. I. A." The same observation applies to *oxidised* instead of *oxidated*.

Experiments with Muriatic Acid.

1ſt. One hundred grains, placed as in the former experiments, but with diluted muriatic inſtead of nitric acid, were entirely diſſolved in a moderate ſpace of time with a diſengagement of hydrogen gas.

2d. The liquor could contain no other metals but thoſe which are capable of decompounding water, v. g. iron, tin, zinc, manganeſe. But there was no iron, for ammonia, Pruſſiate of ammonia, ſpirituous tincture of galls, cauſed no precipitate.

3d. That there could exiſt neither zinc nor mangancſe, the foregoing experiments with nitric acid (Exp. 4) had ſufficiently demonſtrated.

4th. When a drop of liquid muriate of gold was put in the liquor, a beautiful purple precipitate was formed, which is ſo well known under the name of Caſſius's precipitate. Hence tin was preſent.

It would be too long to detail all the experiments I have made. Some pure tin was oxydiſed by means of nitric acid, in order to ſerve as a point of compariſon; ſome was diſſolved in muriatic acid, and precipitated by carbonate of ſoda; and the reſult of all theſe experiments correſponded, as nearly as ſuch chemical trials will allow, with thoſe made with the bougies.

I was led to queſtion, in ſome meaſure, thoſe experiments, as I could not hear, on bending the bougies, that peculiar noiſe ſo well known, and called by the French *le cri de l'étain*; but upon preſſing them between the teeth, a ſenſible noiſe was eaſily perceived.

One hundred grains of the bougies were melted in a Heſſian crucible, and caſt into a mould; the ſurface was not leſs brilliant than the original bougie, but the ingot was leſs flexible on account of its *ſolidity*. It ſeemed to reſemble exactly common tin, and gave the uſual ſound when bent.

From the foregoing experiments it would appear to me, that the flexible metallic bougies are tin, and that they owe their pliability, as well as their want of ſound, to their being hollow. It will appear evident, I believe, that, if chemical reagents can be depended on, they are not a compound metal.

I was much aſtoniſhed, therefore, at the author's claim of *invention*. I am not well acquainted with the ancient hiſtory of Great Britain, but I well know that ſome authors derive the name of Britain from *Bratanac*, which is ſaid to ſignify, in the Phenician language, the *Country of Tin*; becauſe theſe people uſed to come particularly into Cornwall to buy that metal. They ſold it in their turn to the Greeks, who, on that account, called theſe fortunate iſlands *Caſſiterides*, from Κασσίτερος, Tin. Indeed,

Indeed, this claim of *invention* appears to me to place the claimant in the ſame predicament as a ſailor, who on going to ſea for the firſt time, ſhould diſcover the Iſle of Wight, or of a citizen of London, who beginning his travels, ſhould diſcover Brentford.

I ſhould be very happy, Gentlemen, to preſent you with a chemical ſyntheſis of theſe bougies; but Nature has ſecrets which ſhe imparts to very few, and, for the preſent, I am afraid we ſhall be obliged to reſt ſatisfied with the poſſibility of obtaining from the mines of Cornwall, the metal of which they are formed. As to the mechanical conſtruction, we can obtain information from every pewterer.

With reſpect to their utility as bougies or catheters in ſurgery, I will leave it to ſurgeons to determine; my ſole object was to examine whether they were a compound metal, or had any claim to a diſcovery.

No. 60, Great Mary-le-Bone Street,
February 11, 1802.

I am, &c.
F. N. VANDIER.

P. S. I take the liberty of encloſing the following paſſage, as it may tend to elucidate important facts, and as probably few of your readers ſee the Journal from which it is tranſlated.

"Citizen Texier, Surgeon in Chief to the Military Hoſpital of Invalids at Verſailles, has continued the experiments which he had undertaken in concert with Citizens Alibert, Balſac, and Valois, with a view to determine whether the inſertion of the Vaccine virus would guard ſheep againſt the ſcab.* The reſults which he obtained lately, deſerve being publiſhed; he inoculated with the virus of the ſcab two ſheep which had been previouſly vaccinated, and neither was found ſuſceptible of the contagion. Every one will perceive how intereſting it is, for the improvement of rural economy, to collect ſimilar facts, and eſpecially how important it is to repeat and multiply the trials." Décade Philoſophique, An. x. No. 10.

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* Malis Cornipedum of Liger, la Claveleé or le Claveau; a diſeaſe very frequent among ſheep, and which is ſaid to be contagious; it conſiſts in boils or furuncles which infeſt nearly the whole body. Worms are found in the cavity of theſe boils, when they come to ſuppuration; theſe worms come from the eggs depoſited there by inſects, which often find means to creep and penetrate alſo into the noſe and frontal ſinuſes. See Sauvages, Claſs x. Gen. Malis.

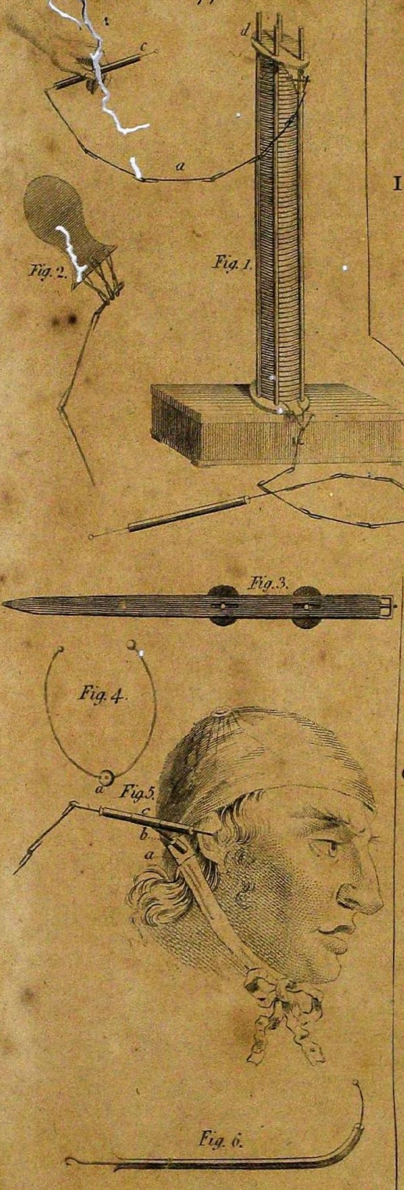
Dr. Augustin, on the Medical Use of Galvanism.

[With an Engraving.]

Dr. AUGUSTIN, of Berlin, author of ſeveral uſeful and judicious compilations, concerning the hiſtory and progreſs of medical ſcience, has lately publiſhed a ſmall pamphlet on Galvaniſm and its medical application; in which, after giving a brief but judicious ſurvey of Galvaniſm, he concludes with relating the phenomena which are produced by the effect of Volta's column or pile on the human body, and pointing out the diſeaſes in which it may be ſucceſsfully employed. We ſhall only communicate to our Readers the laſt part of the above publication, as we have already commenced a more ample hiſtorical account of the Galvanic Diſcovery than we find in Dr. A's preſent work.

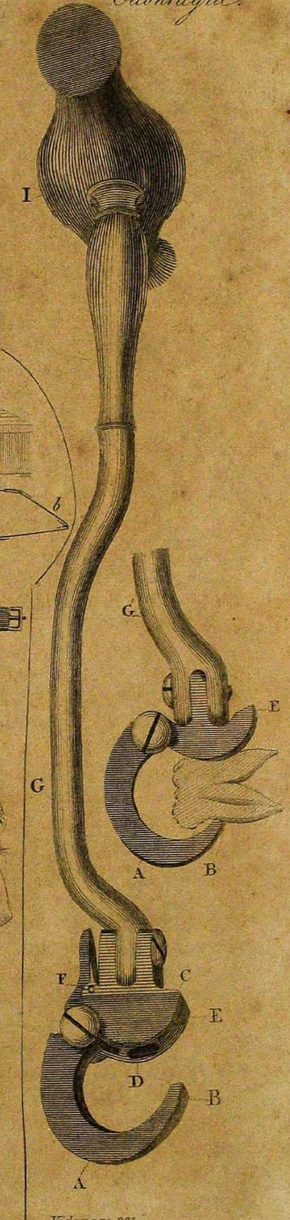
" When a pile is properly conſtructed, ſo as to ſhow itſelf perfectly efficacious, a moſt ſenſible ſtroke will be felt, on touching with wet hands, or by means of ſilver ſpoons, the upper and lower extremities of the pile; the ſenſation, however, does not extend itſelf, nor penetrate into the breaſt ſo much, as from an electrical ſtroke; but when the chain is continued to be ſhut, the ſenſation becomes more permanent and diſagreeable, which likewiſe takes place on putting each chain into a glaſs of water and holding each hand in one of the glaſſes. Theſe ſenſations, however, prove more lively when the pile is freſh conſtructed, but decreaſe the longer it ſtands, particularly when the ends of the pile have been combined with each other by the contact of the two chains. It is however ſingular, that the force of the pile may be renewed and reſtored by ſhaking the chains. The pungent pain perceived on touching both ends of the pile with wet fingers increaſes, if he, who touches them, is inſulated. When ſeveral perſons take hold of one another by the hands, which muſt be wet, and when thoſe at the two ends touch the two extremities of the pile, all of them receive a ſlight ſenſation, which is not ſo ſtrong as in a ſingle perſon, though it becomes ſtronger if they are inſulated. The commotions become evidently more lively on moiſtening the fingers with which the extremities of the pile are touched with a ſolution of common ſalt, but they are the moſt forcible if a part of the touching fingers is deprived of its epidermis. Galvaniſm, applied by means of Volta's pile, ſhows a moſt ſenſible effect on the organs of ſenſe, and particularly on the eyes; for on applying the two wires of the

Galvanic Apparatus with its Medical Application.



Vide page 242. Vol. 7.

Mr. Reeces Odontagra.



Vide page 221.

the zinc and ſilver ſide on the ſkin beneath the eye-lids, which has been previouſly moiſtened with ſalt water, a burning pain will ariſe, and the light be put into a vibrating motion alternately in both eyes, which continues as long as the wires remain at the above place. The ſame phenomenon appears, when we touch with one hand a wire that is placed in a glaſs of water, whilſt we apply the wet eye-lid to the metallic plate of the upper extremity of the pile. The vibration of light, however, is particularly ſtrong on applying one wire on a ſpot above the eye-brow that has been previouſly deprived of its epidermis, and on putting the other wire into the noſe or mouth, as the flaſhes of lightning and the ſtrokes, which then extend themſelves through the whole head, are frequently ſo violent and ſtupefying, as to cauſe lipothymies, when the pile conſiſts of twenty or thirty ſtrata. The other organs of ſenſe are likewiſe evidently affected from Galvaniſm by means of Volta's pile. On bringing, for inſtance, the wire, *a* Fig. 1, into the ear that has been previouſly moiſtened, whilſt the other wire, *b*, fig. 1, is held by the hand in a glaſs of water, a ſtroke and a ſtrong ſound will be perceived in the ear, which becomes more benumbed if the wire *b* is taken out of the water and applied to the other ear, ſo as to bring both into the connexion of the chains. On twiſting wire about the ears, moiſtened with ſalt water, ſo that the ends of the wire can be immerſed into glaſſes of water, in which the chains faſtened at the extremities of the pile are placed, a giddineſs is occaſioned, and moſt beautiful flaſhes of lightning will be ſeen. The ſenſations of ſound, however, and tingling in the ear, become particularly ſtrong when we put a conductor applied to the Euſtachian tube in combination with a chain. On dipping a finger into one of the above glaſſes and into the other a zinc bar, and touching this with the tongue, inſupportable pains will be felt, a flaſh of lightning be ſeen, and a ſingular ſouriſh taſte remain ſome time after. Theſe phenomena, however, are found to differ remarkably by the manner in which the chains are touched, whether by the ſilver ſide or by the zinc ſide of the pile, or whether they are connected or ſeparated; becauſe, in caſe of either ſhutting or diſuniting the chains, the feelings ariſing from the ſilver ſide and that from the zinc ſide are always oppoſite to one another; and what is moſt curious, the ſenſation which is occaſioned on ſhutting the chains at the ſilver ſide is exactly the ſame at the zinc ſide, when the chains are disjoined, while the feeling at the ſilver ſide changes into that which was perceived on the zinc ſide when the chains were ſhut, and vice verſa. Thus, for inſtance, in the ſtrokes perceived in the fingers, the finger at the zinc wire *b*, of

Volta's pile, feels a ſenſation as if a ſtring was tied round it, whereas a diſagreeable pungent feeling paſſes through the finger at the ſilver wire from the point of contact in all directions; but on disjoining the chain this ſenſation changes, ſo that what the finger at the ſilver wire felt, is then felt by the finger of the zinc wire, and vice verſa. When the circle or chain is formed, by means of the tongue, by bringing it into contact with the ſilver wire *b*, fig. 1, of the battery, whilſt one end is applied to the zinc wire a very ſtrong ſhock will be perceived in the tongue, from which an impreſſion remains at the ſpot where the ſhock took place. On forming the chain with the tongue, applied at the ſilver ſide, a very diſagreeable ſhock is produced, which leaves behind it a feeling as if a hole was ſtruck into the tongue. When the tongue is brought in contact with the zinc ſide, and the circle formed by another part of the body, a ſenſation of heat will be felt, together with the ſhock and the ſouriſh taſte, which, however, on disjoining the chains, is changed into the oppoſite ſenſation of cold, and vice verſa, when the tongue is in contact with the ſilver ſide. The ſenſations produced in the noſe differ likewiſe according to the manner of applying the wire; when we apply the ſilver wire, an inclination to ſneeze will ariſe, which is not the caſe with the zinc wire. On bringing the ear in contact with the zinc ſide of the battery, a clear ſound is perceived, which becomes ſtronger when the ear is brought in contact with the ſilver ſide. The phenomena produced in the eyes differ alſo according to the different manner in which the eye is brought in contact with Volta's pile.

From theſe ſtatements, it appears how much the medical uſe of Galvaniſm deſerves to be recommended, and that, notwithſtanding the few practical obſervations which have hitherto been made on that ſubject, we are entitled to expect no inconſiderable advantages from ſo powerful and penetrating a ſtimulus. In employing it, however, for the cure of diſeaſes we ſhould always conſider the topical as well as the general ſtate of irritability, in order to prevent any bad conſequences which may ariſe by applying too ſtrong a degree of this ſtimulus. A ſtrong Galvanic ſhock generally occaſions laſſitude and a kind of lameneſs, which continues for a whole day, particularly if we have for any long time expoſed ourſelves to the action of the battery. Thus, Mr. Rutter, a gentleman to whoſe ingenious experiments we are particularly indebted for many intereſting explanations with reſpect to this ſubject, felt a general indiſpoſition attended with wearineſs and dullneſs in the head, after having expoſed himſelf for a whole hour to the action of a ſtrong battery, which conſiſted of 100 ſtrata. Inflammations

tions of the eyes after continued experiments with light, debilitated insensibility of the tongue, catarrhs after frequent experiments in the nose, vertigo and head ach after violent strokes through the head, and tooth-ach, which always ensue after any experiment being for some time continued at any of the above parts of the head, are the common consequences of Galvanising, which undoubtedly arise from this stimulus having acted too violently on the sound degree of irritability. In diseases, therefore, where a great irritability prevails, attended with debility, we ought only to employ it in a weak degree; but in paralyses from indirect debility we may immediately begin with violent commotions. Regard should also be had to the organ on which we intend to act. The weakest degree of Galvanism is produced by means of two small metallic plates, which are placed near each other on any part of the body, that has been previously deprived of its epidermis.——The diseases in which the application of Galvanism may be attended with success, are the following:

1. *Asphyxia and apparent death.* Mr. Creve has already recommended, as the surest method of examining the real or apparent death of a person, to apply one branch of an arch, consisting of two metals, to a brachial nerve, previously laid bare, and the other branch on a neighbouring muscle, in order to excite powerfully by this simple Galvanic chain the debilitated irritability. The coating of the nerve, however, being not necessary, as a slight wound of the skin is sufficient for admitting the action of Galvanism, small incisions made with a scarificator will fully answer the above purpose. To these incisions the Galvanic power is to be led by means of the two conductors *c c.* fig. 1, which must be applied at a small distance from one another. On perceiving convulsions, we may conclude that there is still incitability left in the body; but when no commotions are produced by the strongest Volta's pile, a total want of vital power must be supposed, provided the battery is in such state as to shew itself perfectly efficacious. The same method may be observed in asphyxia; we ought, however, always to begin with a few strata, and to increase them by degrees with great precaution, for fear of extinguishing the small quantity of vital power that may be still existent. For this purpose, Volta's battery seems by far to be better calculated than any other manner of applying electricity, because the necessary degree of stimulus can be easier adapted to the degree of incitation by Volta's pile; besides, its constituent parts are more portable than an electrical machine.

2. *Paralyses.* Under this great class of nervous diseases, we can only comprehend such as do not arise from organic defects,

fects, as, a pressure of the nerve, a luxation of the vertebrae lumbares, or a kyphosis, but which originate in an internal indisposition of the nerves; whence, for instance, the common hemiplegies from indirect asthenia and any topical debility or inactivity of the nervous system. In order to apply the Galvanism in a paralysis of the extremities, two small spots above the place where the nerve runs, viz. at the thigh above the ischiatic nerve, and at the arm above the nervus cutaneus externus, are deprived of their epidermis; and when they are sufficiently moistened, the Galvanism of Volta's battery is conducted to them by means of the two chains *a b*, and the conductor *c c*, which must be taken by the glass tube and moved on the excoriated places. When the paralysis is attended with a high degree of insensibility, or a total loss of motion as well as sensation, the Galvanism should be applied in a strong degree, in order to excite the nervous power, and to accelerate the process of life by this new and penetrating stimulus; which being done, the number of strata must be afterwards diminished, so that by degrees a less degree of this stimulus acts with the same force as a greater one did before. By observing this rule, I succeeded some time ago in restoring sensation perfectly, and motion for the most part, with causing at first strong commotions by a battery of sixty strata in a hemiplegia from indirect debility, attended with total insensibility, in a patient sixty-six years of age; but as the limb was vehemently convulsed, and became so painful to the patient that she refused a continuation of these experiments, the cure could not be performed merely by Galvanism, but recourse was had to the use of other excitant remedies.

3. *Nervous diseases from direct asthenia*, in which the irritability is so much accumulated, that all stimuli produce too violent and preternatural sensations; of this kind are, spasms, convulsions, chorea St. Viti, but particularly trismus and tetanus. The first application of a powerful stimulus ought naturally to be weak in these diseases, and which is to be done by means of two different metallic plates, which being fastened to a leather strap, are applied at two places, and brought into contact with each other by means of the conducting arch, fig. 4. These places, however, must have been previously excoriated by blisters, or wounded by the scarificator. When no effect ensues on the application of these single plates, we ought to combine them with one another, and thus increase by degrees the number of strata and the force of Galvanism. By immediately beginning with strong Galvanic shocks in these directly asthenic diseases, we might probably do more harm than good; and I saw violent pains and convulsions arise

from

from the application of a ſtrong Galvanic battery with forty or fifty ſtrata, in a patient affected with a continual motion and a great ſenſibility of the lower extremities; whereas the uſe of Galvaniſm, from a few ſtrata, ſeemed perfectly to agree with him. The application of this ſtimulus in caſes of triſmus and tetanus has not yet occurred to me; but it may be ſuppoſed, in all probability, that thoſe diſeaſes ariſing from a total torpor in the muſcles, which originates in the nerves, may be removed by a ſtimulus that ſhews itſelf ſo powerful in ſimilar affections. I ſhould, however, think it adviſable, in this caſe, to increaſe the force of the battery more ſuddenly.

4. *Weakneſs of ſight and amauroſis.* Galvaniſm muſt be employed here with the utmoſt precaution, particularly in the firſt caſe. One of the chains, Fig. 1, *b*, being placed in a glaſs of water, into which the patient is ordered to hold his hand, while the conductor of the chain, *a*, is applied to the eye-lid, which muſt be previouſly moiſtened. The flaſhes of lightning which then appear, become ſtronger or weaker according to the number of ſtrata of which the battery conſiſts; but we ſhould begin with a few ſtrata, and only increaſe them by degrees. In a perfect amauroſis, however, we ſhould uſe a more violent degree of Galvaniſm; to which end, a bliſter being applied above the eye-brows of the diſeaſed eye, near the glabella, the patient is ordered to put a wire, fig. 1, *b*, into the mouth or the noſe at the ſide of the diſeaſed eye, and the conductor of the chain, fig. 1, *a*, is then brought in contact with the place deprived of its epidermis by means of the bliſter. For the beginning, ten ſtrata are ſufficient, which may be afterwards increaſed. Patience and perſeverance are required for performing ſuch a cure, and I ſucceeded in this caſe, though not till after an experiment eight times repeated, in producing ſome change in the diſeaſed eye; for after having increaſed the ſtrata to thirty-five, at the ninth experiment a ſlight glance of light was produced, which is ſlowly augmenting. I have not experienced any quicker effect of Galvaniſm in amauroſis, and I am therefore inclined to think the accounts of amauroſis being cured by Galvaniſm in a ſhort ſpace of time exaggerated, though it may prove of quicker and better effect in the beginning of an amauroſis. There is, however, a kind of amauroſis, which originates from a congeſtion of blood towards the head, where Galvaniſm is by no means indicated, but where topical venæſection, foot-baths, and the application of cold water on the eye are of the beſt ſervice.

5. *Difficulty of hearing and deafneſs.* As theſe affections ariſe from different cauſes, in which Galvaniſm is of no uſe, we can only expect advantage in thoſe caſes where a paralyſis of

of the acouſtic nerve is to be removed, and perhaps alſo where the ſecretion of the ear-wax is to be promoted. In total deafneſs from a paralytic ſtate of the acouſtic nerve, I uſe immediately in the beginning a battery of twenty or thirty ſtrata; but in difficulty of hearing, a leſs degree of Galvaniſm is to be applied. The beſt mode of application in the above affections is the following: A place behind each ear, on the proceſſus maſtoideus, being deprived of its epidermis, a zinc plate is applied on one ſide and a ſilver plate on the other, by means of the bandage repreſented in Fig. 5. This bandage conſiſts of a hoop of whalebone, which is faſtened by means of a cap round the occiput, and of a ribband tied under the chin; on the two ends of this hoop we can ſcrew the plates, fig. 5, *a*, which being brought in combination by means of a ſilver chain are ſuffered to lie for ſeveral days: A humming ſound ariſes now in both ears, and an acrid ſerum iſſues from the ſore places, particularly from that touched by the zinc plate, which ſtrongly calcines, and on account of its violent effect muſt be frequently changed with the ſilver plate. This mode of applying a ſimple Galvanic chain is frequently efficient enough, as a powerful ſtimulus, in deafneſs from metaſtaſes of a morbid matter, and in that kind of difficult hearing which ariſes from a dryneſs of the internal ſurface of the tympanum, to be diſtinguiſhed by the patient's hearing better through the mouth than through the auditory canal, and by the dryneſs of the membrana pituitoſa of the noſe. In that kind of deafneſs, however, which is attended with a want of the ear wax, it is rather adviſable to bring the conductors of Volta's battery, fig. 1, *c c*. into the auditory canal, which muſt be previouſly moiſtened; for this purpoſe we can make uſe of the apparatus, fig. 5, *b*. conſiſting of a braſs cylinder or nut, fig. 5, *b*. over which the conductor is moved into the ear as far as we think proper. The ſtrongeſt action of Galvaniſm, on the organ of hearing, is produced by bringing a curved wire, fig. 6, into the Euſtachian tube, which being put in connection with one chain, the patient is ordered to hold the other in his wet hand.

6. *Aphonia and hoarſeneſs*. Theſe affections may alſo originate from different organic leſions, viz. abſciſſion of the nervus recurrens, &c. in which Galvaniſm is of no ſervice; but where obſtructions in the glands and a paralyſis of the above nerve, or of the muſcles of the tongue, take place, it proves undoubtedly of great effect. To this end a leather ſtrap, fig. 3, may be uſed, by means of which the zinc and ſilver plates are faſtened on two ſpots, deprived of their epidermis above the muſculus ſternocleido maſtoideus, near the larynx: the plates are brought into connection by the conducting metallic arch, fig. 4, which

which can be widened and ſhut at *a*. The effect of Galvaniſm may be increaſed by applying the two conductors, fig. 1, *c c*. Dr. Grapengieſſer † ſucceeded in this manner in removing, in the ſpace of twenty-four hours, an aphonia that had laſted ten years.

7. *Chronical rheumatisms with stiffness of the limbs and joints.*

8. *Cold swellings and asthenic inflammations.*

9. *Tooth-ach.*

10. *Oedema and dropsy*; in which the activity of the abſorbent veſſels is to be reſtored or increaſed.

On the whole, we may avail ourſelves of Galvaniſm in any aſthenic diſeaſe; but we ſhould always regard the difference between direct and indirect aſthenia, if we intend to do any good by the application of that powerful ſtimulus.

Explanation of the Plate.

Fig. 1. Volta's battery between four glaſs pillars, with the diſcharging chains, *a b*, and the conductors, *c c*, faſtened to them, which conſiſt of wires paſſing through glaſs tubes.

Fig. 2. One of the braſs plates, which are placed, one on and the other under the pillars, and to which the chains, *a b*, fig. 1, are faſtened.

Fig. 3. Two metallic plates, the one of zinc the other of ſilver, which are faſtened by buttons in a leather ſtrap, to be applied in aphonia and hoarſeneſs.

Fig. 4. The conducting arch, by means of which the connexion between the two plates is formed.

Fig. 5. A bandage for the head, for the application of Galvaniſm in caſes of deafneſs, conſiſting of a piece of whalebone to paſs over the back of the head, which is to be faſtened by a cap on the head and a band under the throat; to the two ends of which two plates of different metal can be ſcrewed over the proceſſus maſtoideus, and the apparatus above deſcribed.

Fig. 6. An inſulating wire paſſing through a glaſs tube, the upper end of which is provided with a button, and conſiſts of fine ſteel, in order to bring it conveniently into the Euſtachian tube.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

THE *laſt* number of *The London Medical Review and Magazine*, having given a general invitation to its Correſpondents in the Magazine department, to tranſmit their future Commu-

† We purpoſe to lay before the public, in a future number, the experiments of this gentleman with Volta's battery.

nications to the *Medical and Physical Journal*, (as the former work was only to be continued as a Review) I take the liberty of availing myself of this offer to transmit the following particulars of *a Case of Cow-pock, which was falsly stated in the Bills of Mortality for* 1800, *to have proved fatal*, but the fallacy of which statement I have been enabled to ascertain.

A series of protracted enquiries having enabled me to trace this Case to Lambeth, Surry, I was favoured by the intelligent and respectable Mr. Gawler, of that parish, with a direction to the person who nursed the child, (in China Row) and from whom I received the following account.

"The child was a son of Mr. W. at Knightsbridge, and at the age of between four and five months was inoculated with Cow-pock, by Mr. C. of the above place. This failed, and the operation was repeated in six weeks afterwards, and *with effect*. The pustule arose, and proceeded in the usual way, as to time, encrustation, &c. Mr. C. did not see the child during this time, telling the nurse, very properly, that it would not have any illness, or require any medicine. The pustule disappeared in the usual time, and the brown encrustation was one day found in the night dress (or bed gown) of the child, who continued in good health throughout the whole process, as usual."

"TWO MONTHS *afterwards*, an eruption (so common with infants) appeared on the chin of the child, which was "*supposed*" to be occasioned by an increased flow of the saliva, in consequence of dentition, and afterwards on the lower extremities, as far as the tendo Achilles, where it was "*attributed*" to an excoriating effect of the urine; after this it became visible on the upper extremities, the trunk of the body remaining free from it, but one of the eye-lids was also affected. Its appearance was that of a scald, with a scurfy surface; and it was "*thought*" to be of a scorbutic nature. This eruption was "*supposed*" to affect the stomach, and to occasion spasms in that organ. It did, however, nearly disappear in about *two weeks*, and the child was thought so well as to be reported so to its parents.

"*At this time*, convulsions were excited by the process of dentition, and in a fit of this kind," *(perfectly unconnected either with the inoculation or the eruption)* "the child died."

"No idea whatever had been entertained of any connection between this child's death and the *Cow-pock*," (as indeed it would be difficult to suppose) "until the assertion was made by Mr. H. of F—— street, Soho."

From this, therefore, arose the error; for when the sapient Searchers came to enquire *the cause of the child's death*, the nurse

nurse very justly and cautiously answered, that two medical gentlemen had seen the child, and one of them said, the death was occasioned by the *Teething*, and the other, that it was the consequence of the *Cow-pock* inoculation; from which latter assertion, as most unfounded, but probably the most congenial with their ideas, the report was made and entered, *for the information of the public*, and their instruction in a lie!* So that the origin and propagation of this error arose with Mr. H. THE CHILD DIED OF CONVULSIONS, IN CONSEQUENCE OF TEETHING, NEAR THREE MONTHS AFTER ITS INOCULATION. Of the necessity and importance of paying a rigid adherence to TRUTH, and of having the reports made by persons of real information and medical science, this example is an abundant proof. I, myself, can also witness how obstinate an error it produced in the minds of many individuals, who did afterwards, perhaps, severely regret its propagation, but from whom it was with difficulty removed at the time.

Dr. Jenner, I believe, was aware of this circumstance, as he stated it to me from the information of Dr. Ludlow, who, as the nurse informed me, had been making the enquiry of her; but of the particulars, as above related, none I believe have yet transpired to make the FACT *public*. I am happy therefore in being thus enabled to communicate the *true state of the case*, and thereby to comply also with the request of Dr. J. Hoping therefore it will thus answer its intended purpose, I remain,

Your's, &c.

Rockingham Row, Newington Butts.
Jan. 27, 1802.

C. PEARS.

On VACCINE INOCULATION.

By Dr. COGAN.

THE following extracts from a letter on Vaccine Inoculation, communicated by several medical gentlemen in Rotterdam to the Agent of the National Institution, appear to me not

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* It is a well-known FACT that the report of the Searchers, however obviously absurd and false, *cannot* be altered, nor dare any person *presume* to do it. Neither is it long since that these arbiters of disease, impudently reported the death of a Medical gentleman's child to have been occasioned by a malady the very reverse of fact; which, when he explained, they insolently replied, that "though he was a Doctor, he knew nothing about it." Their report was received; and when the gentleman applied at the office of the parish clerk, to have the error rectified, he was told, *it could not be done!* Excellent source of information.

not unworthy the attention of the Engliſh reader, as their experiments were made in a manner peculiarly ſatisfactory, and as ſome parts of the information are both novel and intereſting.

In the month of October, 1799, Mr. Davids, a practitioner in Rotterdam, in conjunction with M. Duvigneau, made their firſt experiment with matter received from Dr. Walker, which ſucceeded to the utmoſt extent of their wiſhes, and encouraged theſe gentlemen and ſeveral others, animated by their ſucceſs, to inveſtigate the ſubject more minutely. They made ſeveral attempts to procure matter of the Cow-pox both from France and England; but theſe proving abortive, from a concurrence of circumſtances, Mr. Davids undertook a journey to Paris, not only to procure matter, but to obtain information concerning whatever related to the practice. Upon his return he encouraged our deſigns, by aſſurances that the experience of the practitioners at Paris perfectly coincided with the reports received out of England. But neither the Vaccine matter which he brought with him, nor that ſent to us from England, proved ſucceſsful. At length we received four lancets, charged with the matter from the Inoculating Hoſpital in London, and, at the ſame time, an infected thread from Dr. Nowel, at Bologne. With theſe, ſeveral children were inoculated, and we had the ſatisfaction to ſee that it ſucceeded with three in the moſt perfect manner; which cheriſhed the joyful proſpect, that the *Vaccina*, having once taken root among us, would in proceſs of time ſpread itſelf over the country, and annihilate one of the moſt dreadful calamities to which humanity is ſubject.

To detail all the circumſtances attending their various experiments, would be an unneceſſary repetition of what has been ſo repeatedly communicated to the public, I ſhall therefore confine myſelf to a few of the moſt intereſting.

"We found (ſays the writer) that the matter for inoculation muſt be taken as ſoon as it appears in the wound; and that it was not proper to wait longer than three days, generally from the eighth to the tenth; for as ſoon as it became thick, and of the conſiſtency of *pus*, we found either that the inoculation did not take place, or it produced a ſpurious pock, which was no ſecurity againſt the natural infection.

"Though, in general, the progreſs of the wound was perfectly ſimilar to that with Variolous matter, yet we found that it was much ſlower in ſome caſes; ſo that the wound did not exhibit any marks of activity before the ninth, tenth, and even the fourteenth day after the inciſion.

"In order to obſerve the ſimilarities and differences more minutely, we inoculated with the natural Small-pox at the ſame time,

time, and under the ſame circumſtances; and we found that the ſwelling, and production of matter in the wounds, were ſo ſimilar both in appearance and in time, that the moſt experienced inoculator could not diſcover any difference until the tenth or eleventh day.

" We obſerved that the Cow-pox did not require, either before or after inoculation, any change of regimen, which was the caſe with the Small-pox.

" About the eighth day (in ſome earlier, in others later) moſt children who were able to expreſs their feelings, complained of a degree of pain under the axillæ; and this was followed, on the ninth or tenth, with a great quickneſs of pulſe, and redneſs on the countenance, which, in ſome inſtances, was attended with a redneſs over the whole body. Theſe ſlight appearances of fever were accompanied with a loſs of appetite and general indiſpoſition; but theſe are not to be compared with the effects of Variolous matter at the ſame period.

" Of the numbers that were inoculated, ſome of the children were weak and ſickly; one was ſubject to fits from its birth; but in none of theſe were their diſorders increaſed; and in the child ſubject to convulſions, we found that theſe were much leſs during the inflammation of the wound. In ſome that had cutaneous diſeaſes, ſuch as the ring-worm, &c. the eruptions either totally diſappeared, or were much diminiſhed. In theſe ſubjects the *cruſt* generally fell off the earlieſt, and left an obſtinate but harmleſs ulcer.

" A perſon who had been inoculated with the Cow-pock four days, indicated ſymptoms of the natural infection; and theſe put on the appearance of the *confluent* kind; the ſkin was covered with eruptions; but, in proportion as the Vaccina advanced, thaſe eruptions became *diſtinct*, and diminiſhed aſtoniſhingly in number. The eſchar, or cruſt, remained a length of time after the diſeaſe had terminated.

" In twenty caſes Variolous inoculation was ſuper-added, without producing the diſeaſe. In all theſe, the wounds were inflamed on the firſt or ſecond day; on the third they were more inflamed and ſwollen; and, in ſome inſtances, to ſuch a degree that we began to ſuſpect a general infection would ſucceed; but theſe were, on the fourth day, leſs inflamed and ſwollen, and they diſappeared on the fifth.

" One of our inoculated patients was placed by its father in bed with another child that had the natural Small-pox to an alarming degree, and continued in this ſituation for ſeveral days without experiencing the leaſt effect. One of our ſurgeons, Mr. Philipeau, whoſe daughter was *vaccined* under our inſpection,

tion, permited the child to embrace a patient in the *confluent* disease, just at the period of maturation. The patient died, but the child received no injury.

"We have observed, in general, that a single pock manifested itself upon each wound; but, in some instances, we observed one or two others in the vicinity. In one case we counted about seventy small festering pocks surrounding the large one, and scarcely to be distinguished before they dried up from appearances in the Variolous Inoculation. The scale of the large pock was of a much darker colour than the others; these fell off together, left an inflamed wound which yielded pus, and continued for a long time; but was at length completely cured.

"In one patient we remarked, that after the twelfth day, when the crust fell off, leaving a large quantity of matter in the wound, several pocks appeared upon one cheek. Whether or not these were communicated by the mother, who by putting on the cap after dressing the wound, might have infected that side of the cheek where she fastened the band, we could not ascertain. It is certain, that in another child, where no superinfection could take place, Vaccine eruptions, about five or six, appeared on the extremities; and in a third, a small number was spread over the body. We inoculated with the matter taken from the cheek of the first, which produced the genuine Vaccine pock. A woman who dwelt in the same house with this child, and who had had the Small-pox in early life, by frequently embracing the child, had Vaccine eruptions in her face, manifestly of the spurious sort.

"Seven days after the inoculation, one of the patients caught the measles, which were at that time prevalent. The pock already formed was checked in its progress for the space of fourteen days, excepting that it became somewhat enlarged, until the measles were terminated, which was on the twenty-first day after inoculation. The Cow-pox now began to acquire activity, formed the areola, and became dry on the twenty-fourth. The measles were of a very unfavourable kind; and it appeared to us highly probable that the patient would have fallen a victim to its malignancy without the aid of the Cow-pox, which, though apparently stationary, acted as a stimulant to the system. In this it resembled the natural Small-pox, and suggested to us an idea of the possibility that the Small-pox might, in its origin, have been a *Vaccine* malady; which, in consequence of various changes and modifications, degenerated in the human body until it acquired its known malignancy. Farther discoveries may enable us to confirm or reject this idea.

"We

"We have been indefatigable in our Inquiry, whether the Cow-pox was a malady known among the cattle in the Netherlands; but have not as yet acquired ſatisfactory information.

"Signed, TH. F. VAN OPDORP.
L. DAVIDS.
F. H. GRAM.
I. F. DUVIGNEAU.
C. VAN HATTEM."

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE Medical World is indebted to you for a Publication, which enables it to know every remedy propoſed, whether the ſame plan has been adopted by other practitioners, and with what advantage, and to judge of the effect by an accumulation of experiments; therefore, as you have inſerted in your valuable Miſcellany a Letter addreſſed by me to the Printer of the Glocester Journal, ſigned A MEDICAL PRACTITIONER, I may be expected to explain my idea of the effect produced by waſhing with Vinegar.

I conceive that the effluvia (which I have, perhaps, incorrectly termed human atmoſphere) generated and thrown out upon the ſkin during the progreſs of Fever, has putrid and alkaline properties, which, when ſeparated from and ſurrounding the body of the patient, become one moſt material ſource of communicating the diſeaſe; conſequently, vinegar being antiſeptic and acid, when combined immediately (at the ſource) with this effluvia, produces an inert neuter, whereby the progreſs of Fever is checked from extending the contagious influence amongſt the attendants, and the type of fever, if not ſometimes altered, is meliorated in the ſymptoms and violence upon the patient.

By well rubbing after the vinegar is uſed, the ſkin is perfectly cleared from any remaining impurity, and a degree of perſpiration enſues, which is always comfortable to the patient. If indigent circumſtances will not afford the purchaſe of flannel dreſſes, I adviſe a renewal of blankets or ſheets, well aired, and the bed warmed every time after the vinegar is uſed.

I was induced to recommend the uſe of vinegar through the medium of the Gloceſter Journal, for the benefit of our neighbouring poor; and by this practice amongſt them, I have experienced

perienced every advantage; I find their habitations more free from filth; I think I have less reason to dread contagion; and I observe that those who were liable to the attack of the same fever, (from being obliged to sleep in the same bed, or in the same room, with the sick) continue in health.

Is it not probable that, in times like the present, nothing would so speedily exterminate or prevent fever in Poor-houses, Houses of Industry, Prisons, &c. as a general washing of every individual once or twice in a week with strong vinegar?

Anxious to be serviceable in that profession of which I am proud to call myself a member, I beg to subscribe myself,

Your's, &c. &c.

Glocester, Jan. 10, 1802. CHARLES COOKE.

P. S. The power of communicating the contagion of fever appears to be very limited, excepting by immediate contact; it does not seem, at farthest, to extend beyond the sick-room; I do not think it very diffusive in the air. I have attended patients with fever in almost every house in one parish in this county, and the surrounding parishes which adjoin have hardly an instance of the same disease. In another parish, (on the opposite side of Glocester) a woman was taken ill with fever; the eldest daughter was very soon attacked in the same manner; I then pronounced the fever contagious. From this time the neighbours would not go near the house. Three other children, with the woman's husband, became ill shortly afterward: The whole family, however, recovered; and I cannot learn that the fever extended itself farther in the parish.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I Am one among a great number of the Readers of your excellent Medical Miscellany, who are doubtless very much obliged, and highly entertained, by the hints that from time to time we meet with in the Physical Journal, concerning the medical practice and useful discoveries of foreign as well as of domestic practitioners in the noble art of healing. But altho', in general, I wait with some degree of impatience for the monthly appearance of the communications of your ingenious and instructive Correspondents, together with the result of your learned lucubrations, it was not till this moment that I observed

it

it announced (see page 378 of your last volume,) as a practice peculiar to Prof. HUFELAND, to employ clysters of lime-water, to free a patient from those small troublesome white worms called ascarides, a species the most frequent and dangerous in this country, and which sometimes produce the most alarming and fatal symptoms.

Happy as we ought always to be, to recognize the merit of ingenious and communicative foreigners, I cannot forbear wishing that your readers may be informed that Professor Hufeland's method of removing ascarides, is neither "*new nor rare*;" for, more than ten years ago, I had the honour to hear that skilful and eloquent teacher, Mr. CLINE, when delivering Lectures on Anatomy at St. Thomas's Hospital, recommend the simple but efficacious remedy above mentioned, to be employed in the same manner, and for the same intentions.

In two instances that fell under my own observations, when clysters of lime-water alone did not appear to be so effectual, in every other instance in which I had an opportunity to employ them, I directed a small proportion of *tinctura aloes composita* to be added to the *aqua calcis*; in the other case, complete success attended the use of clysters of lime-water prepared with a decoction of the bitter herbs or shrubs, as *absinthium vulgare*, *Abrotonum*, *et Matricaria*.

Strong lime-water thus prepared, (or the herbs may be added to the lime previously to pouring the boiling water upon it,) when thrown over a garden by a watering pot, is one of the best preventives of the ravages that are often occasioned by slugs and caterpillars. Garden slugs are also killed by urine that *has been kept two or three days*; but that cannot be safely thrown near the roots of plants or trees, nor upon them, though it is extremely useful to prevent the breeding of vermin, when it is thrown on dung-hills with which the ground is to be manured, but lime-water prepared with a strong decoction, or by infusing in it the bitter plants, or shrubs, will, without injury to vegetation, be found eminently serviceable for destroying those rapacious insects.

The lovers of Medical Botany, and those who have a taste for the pure pleasures of Horticulture, will, I trust, with yourselves, Gentlemen, excuse this digression of

Your's, &c.

JOSEPH BROWN, M. D.

Queen's Lane, Lower Street, Islington, Dec. 10, 1801.

To the Editors of the Medical and Physical Journal.

Gentlemen,

Accident prevented my seeing the 34th Number of the Medical and Physical Journal till within these few days, or I should certainly have answered the queries of Mr. Ward, respecting the case of John Bayley, inserted in your Journal for February, 1800, in which I had successfully used the opiate friction.

I am happy that Mr. Ward's questions gives me now an opportunity of correcting an error of the press with respect to the quantity of camphire and oil which was used in each portion of the liniment in the case of Bayley. The following was the quantity used at each friction, viz. camphire half a drachm, ol. oliv. two drachms, the tincture of opium *only* was increased or diminished as circumstances indicated.

In answer to the other questions of Mr. Ward, whether any syphilitic symptoms appeared since the bubo was healed? and whether mercury had been employed? I can only inform him, that during the period he continued in the hospital after his recovery, no venereal symptoms whatever appeared; but he had taken mercury to a considerable extent previous to his admission into the hospital, but as it was exhibited during the time that he was in Holland, and partly on board a transport, where it was impossible proper attention could be paid to him, I attributed the bad state of his health and the sphacelating appearance of the bubo principally to that circumstance.

From being much emaciated he became in a very short time after his recovery extremely corpulent, and left the hospital to join his regiment in the best possible state of health. I have since that heard no more account of him.

The opiate friction was used in a few similar cases with that of Bayley, but not with the same *striking* advantages, yet was of evident service in arresting the spreading of gangrene, and allaying irritation. Lately I have had no opportunity of trying its efficacy further, but am convinced of its good and powerful effects.

I sincerely wish Mr. Ward every success in his indefatigable endeavours to ascertain the modus operandi of opium when introduced by friction, and he may depend upon my communicating the result of any future trials I may have an opportunity of making with it.

I am, &c.

Dover, Feb. 7, 1802. THOMAS DOCKER.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

WHEN I ſent two Caſes of Apoplexy for inſertion in your uſeful Journal, it was with a profeſſed deſign not to enter into any thing Controverſial on the ſubject then in debate. I took it for granted that other Correſpondents, of ſuperior endowments, would be excited to communicate their ſentiments on a diſeaſe ſo generally formidable in its conſequences to the human frame; and herein I was not diſappointed in your laſt Number. — Controverſial ſubjects, however unpleaſant in one point of view, are generally attended with this advantage, as they excite others to a cloſer inveſtigation of that which is *dubious* on the point, and not unfrequently to reflect light on the obſcurity in which they are involved; thereby aiding us more ſucceſsfully to treat the variety of perplexing caſes which unavoidably fall to the lot of Medical Practitioners.

It is very deſirable that ſubjects of medical diſpute ſhould be managed without perſonal animoſity, and as devoid of acrimony as poſſible. This would evince that the Diſputants were more emulous to ſerve the general cauſe, than to gratify their private feelings; which, on ſuch occaſions, are not always under the moſt favourable influence. All will allow, that the Science, however improved, has not yet arrived at its *ne plus ultra*; there is, therefore, ample room for the exerciſe of candour and diffidence in treating of ſubjects, particularly of the more ambiguous kind. Reſpecting my obſervations on the exhibition of *Emetics* in caſes of Apoplexy, which Mr. Crowfoot ſaid, "might have been ſpared," I did not wiſh it to be underſtood as applicable *particularly* to the Caſe then in diſpute, but, in a general ſenſe, as unfavourable where there are ſymptoms of compreſſion on the brain; in which ſentiment I have the concurrence of moſt practitioners; it ought not, therefore, to be miſconſtrued into perſonal reflection: And as to Mr. C's pamphlet on the ſubject, I doubt not but he has made uſeful remarks on the variety of appearances the diſeaſe in queſtion puts on; and when it falls into my hands to peruſe, it will be without prejudice, and, probably, with ſome profit.

I am, &c.

Piccadilly, Feb. 5, 1802. H. DAVIES.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN the Science of Medicine there has ever been, and perhaps ever must be, much difference of opinion; yet, as by the collision of facts, sparks are often produced to light us in our search after truth, a *temperate Controversy* may generally be productive of good. But the issue of *invective* is widely different; it gives birth to tortuous assertions, to personal reflections unworthy the man of science, and engages a place in the mind which would be much better occupied by the subject in dispute than by any reference to the contending parties.

I have been an attentive reader of your Journal from its commencement; and although I have, in common with others, observed it contained several things deserving of censure, yet does it afford much to commend. I could not, therefore, peruse with indifference the *illiterate* and *illiberal* personalities which have lately appeared in it. I am altogether unacquainted with any one of the parties; I attack not the *men*, but the *sentiments* they have published. I am *inimical* to personal reflections in whatever manner they are published; but I *detest*, and will ever lash, the production which teems with *unprovoked* obloquy; — nor can I altogether exclude the Editors from a share of the blame. They will, perhaps, urge in reply, that the *Name* of the writer fixes all the evil on himself; or, though they publish some things anonymously, they are in possession of the Author's real name. This answer does not satisfy me; *invective* on all occasions, and all *personalities* whatever, should be banished from your pages. You have the power, and you ought to exercise it. Such prunings may offend a few irritable Correspondents, but that is far better than to disgust the most respectable of your Readers by the admission of scurrilities. I would wish to propose the following Queries to your Readers, if you deem them consistent with the above sentiments.

1. Has Philo-Medicus been refused literary honours, or does he consider it as a necessary proof of his *literary candour*, that he should scatter his obloquy upon *every* Member of an University, whatever may have been the order of his admission to its honours, or however amply he may have been qualified to receive them in other approved literary seminaries?

2. Does it consist with the dignity, duty, or honesty of a Physician, to prescribe in Hieroglyphicks, the key to which is in the exclusive possession of one man, and consequently unintelligible to every other member of the Profession? *

3. Is

* We do not know what this Query alludes to. EDIT.

3. Is not your Communication from Abingdon,† a palpable breach of that *modesty in controversy* which it would ſeem inclined to inculcate, when it charges a Phyſician with *impetuosity*, *pride*, and *confidence*, becauſe he acts upon principles which *he* believes to be derived from good and ſufficient premiſes?

4. Is a Phyſician, or any other duly qualified Medical Character, to be charged with arrogance,‡ becauſe he dares to think for himſelf, although *all* he ſo dares to think may not be correct?

5. Should not the animadverſions of every Medical Controvertiſt be directed to the *ſubject matter* only, and without any regard being had to the *Authors* of opinions in diſpute?

6. If criticiſms be admitted as to the *manner* rather than to the *matter* of philoſophic communications, in what Number of the Journal will ſome of its Correſpondents criticiſe themſelves?

I am, &c.

Norfolk, 6 *Feb.* 1802. ARISTARCHUS, Jun.§

Inquiry into the Influence of Chemistry on the Operation of Animal Bodies.

An abridged Translation of a Treatise written on that Subject in the Dutch Language, by Dr. Ontyd, *Physician at the Hague.*

Among the different Arts and Sciences, in which conſiderable progreſs has been made during the laſt century, we may doubtleſs place Chemiſtry in the firſt rank. What an aſtoniſhing difference do we find in the ſtate of our knowledge reſpecting this important ſcience, between the commencement and the cloſe of the laſt age! That we are much better acquainted with the component parts of natural bodies, the changes that take place in conſequence of their union and influence upon each other, and the operation of chemical laws upon animal bodies, than our anceſtors were, is a fact that no one can diſpute: Therefore, it is not ſurpriſing that, in the preſent day, frequent attempts ſhould be made to render the diſcoveries in this

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† Vol. vii. p. 153—155. ‡ Ibid. p. 139.

§ We can aſſure A. that a numerous claſs of our readers are ſo far from agreeing in ſentiment with him, that they ſay, "If you reject the ſalt of controverſy, your Ragout will ſoon become as inſipid as thoſe of your predeceſſors."

this important ſcience ſubſervient to the different branches of medicine, and employ them to explain various appearances in animated nature; nay, there are ſome who have ventured to attribute the firſt principles both of inanimate and animated bodies, and all the appearances of the latter, to the union and affinities of the different materials of which animated bodies are compoſed. Reil, who, with Galvani, may be conſidered as the founder of this new chemical ſect, propoſed his ſyſtem with a degree of caution, and ſimply maintained, that moſt of the appearances in living bodies were principally to be aſcribed to the chemical properties of animated matter. He does not attempt to apply his hypotheſis to the practice of the healing art, but ingenuouſly confeſſes, that ſince we are as yet ignorant in what manner the mixtures in organized bodies are changed by their action upon them, we ought not to truſt to any theories in the cure of diſeaſes, but prefer that mode of practice which experience has diſcovered to be efficacious.

But this cautious manner is not equally apparent in the writings of his followers; they not only aſcribe every appearance in living bodies, without exception, to ſome chemical cauſe, but they attempt to explain the nature of diſeaſes, and introduce remedies, upon the principles of chemiſtry alone. Thus Girtanner, Beddoes, Cruikſhanks, and others, maintain, that the venereal diſeaſe owes its origin to a deficiency of the acidifying principle (oxygen), and attempt the cure by ſupplying this defect. Dr. Trotter aſcribes the dyſentery to the ſame cauſe. Dr. Rollo proceeds ſo far, as to aſcribe idiopathic diſeaſes to a ſuperabundance or a defect of oxygen: He has accordingly formed a conciſe Materia Medica, by means of which, according to his ſyſtem, a *plus* or *minus* of oxygen may be adapted to the particular nature of the diſeaſe; and he has lately publiſhed, by order of the King of Pruſſia, a *chemical mode* of curing fevers. Profeſſor Reich alſo preſents us with a ſure remedy to cure every ſpecies of fever very expeditiouſly; for as all fevers, according to him, proceed from a leſſer or greater defect in the requiſite quantity of acid matter, thus are they to be cured by the ſame kind of remedy, which conſiſts of the plentiful uſe of acids.

But, notwithſtanding a prudent application of our chemical knowledge to the human œconomy promiſes great advantages both to the theory and practice of phyſic; notwithſtanding that the diſcovery of a new remedy, calculated to alleviate the miſeries of mankind, is moſt deſirable to every humane practitioner; yet, on the other hand, diſcretion requires that no novel remedy ſhould approach a ſick-bed, until we have fully inveſtigated the principles upon which its boaſted efficacy is founded, and

and the reports of its advantageous application have been aſcertained to be genuine. Such precautions become peculiarly neceſſary at a period when foreign practitioners crowd upon us ſuch numbers of new theories, and new modes of practice, of which the majority are found by experience to merit our contempt.

Theſe conſiderations have prompted me to examine how far the actions of animal bodies, either in a healthy or diſeaſed ſtate, are limited by the Laws of Chemiſtry; I ſhall conſider this queſtion both as it relates to Phyſiology and Pathology.

Firſt, I ſhall endeavour to trace to what extent the motions of animal bodies in a healthy ſtate are influenced by chemical laws; and then, ſecondly, enquire into the degree of influence theſe laws exert, either in increaſing or in changing the nature of the diſeaſes to which animal bodies are ſubject, and examine from facts, what aſſiſtance chemiſtry has hitherto afforded us, to diſcover the nature of diſeaſes, or the methods of cure.

As no one among the ſupporters of this new ſyſtem has advanced farther in the application of chemical laws to the Phyſiology of the human body than Prof. Reich, the firſt paragraph of his ſyſtem ſhall ſerve us as a clue to direct our enquiries.

The Profeſſor begins his Treatiſe in this manner: "Every one who views with open eyes, and unprejudiced mind, the appearances in the human body, will feel himſelf compelled to acknowledge, that all its motions of every kind, are the reſult of an uninterrupted chain in the operations of animal chemiſtry, whereby a conſtant change is produced in the mixture of organic matter. Theſe animo-chemical operations are the effects of an inceſſant counteraction of oppoſite powers or principles againſt each other, without which, nothing throughout all ſpace can exiſt, or be ſuppoſed to exiſt. The perpetual compoſitions, changes, ſeparations, of different materials, their mutual correſpondence reſpecting quantities and qualities, the difference of the various organs in which theſe changes are produced, and their ſpecific actions, muſt inevitably occaſion a great diverſity of effects."

Theſe ſtrong aſſertions are totally unſupported, either by argument or facts. He aſſumes it as an indubitable axiom, that all animal functions are founded on the mixture of organic matter, and builds upon this all his ſubſequent aphoriſms, which are in fact no other than inductions from his firſt propoſition. Whether theſe be ſo obvious to every unprejudiced eye, we ſhall leave to the judgement of our Readers. We are not aſhamed to acknowledge the great imperfection of our own optics, and to declare openly, that the whole of what Reil has advanced from the writings of Humbolt and Ritter, whence he

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has obviously borrowed his syftem of fevers, has not been sufficiently efficacious to difpel the mift from our eyes. It ftill remains extremely doubtful with us, whether the firft principle or caufe of vitality, is to be found in any particular combination of animal matter.

Notwithftanding the full conviction we entertain of the advantages which may be derived to the different branches of medicine by the difcreet application of chemical fcience to the animal œconomy; notwithftanding we freely acknowledge that many appearances in organized bodies are happily explained by the application of chemical principles, yet we cannot admit the affertions of Reil, Reich, Schelver, and others, that all the operations of the human body are derived from particular combinations of organized matter, and that the vital power itfelf is merely an effect of the organization and peculiar combinations of animal matter. It appears to us that, in this cafe alfo, the medium is the fafeft road: For though we admit that thefe have a powerful influence in the various operations of animated bodies, nay, that they contribute to the fupport of the vital powers, and that changes in the one are frequently productive of changes in the other, yet we cannot allow that fimple organization is the bafis and caufe of every appearance. The *vital power* appears to us totally different from *organization*; at leaft this opinion appears much more favourable to the explanation of the phenomena in animated bodies. Let us but compare *dead* with *living* bodies, and what an aftonifhing difference prefents itfelf. In unwrought nature, matter is paffive; and left entirely to itfelf, is not fubject to the leaft change. Whenever it is brought into action by fome external caufe, this action perpetually decreafes, until it is totally ftopt; it returns gradually from the ftate of activity to which it was compelled, to a ftate of reft, in which it remains.

Whenever one mafs of matter is made to act upon another, the one lofes its motive power in exact proportion to the degree in which the other partakes of it; fo that the quantity of motion excited, folely depends upon the quantity of power received.

When a chemical combination takes place between two fubftances, they both lofe by this union, fome, if not *all*, of thofe properties they feparately poffeffed, and form a new fubftance different from either.

This lofs of motion, and this change of properties in dead matter, are perfectly uniform and fubject to the general laws of Nature. The laws of mechanic motion regulate the firft, thofe of chemiftry the laft; fo that an experienced mechanic, or chemift, when they can depend upon the goodnefs of their materials,

als, will always be able to know, *a priori*, what will be the refult of their experiments.

But in living bodies, the cafe is far otherwife. Organized matter, it is true, may experience a diminution of motion, but not complete reft. The vital actions of the organs themfelves, the perpetual changes that take place in their conftituent parts, the inceffant action of different ftimuli upon organized bodies, banifh entirely the idea of perfect reft.

The common properties of living fubftances differ effentially from thofe of inanimate matter, The changes which the firft continually experience, the different capacities and powers poffeffed by the numberlefs living beings which fill the univerfe, and thofe particularly which relate to their forms and modes of exiftence, plainly manifeft that they are governed by their own exclufive laws, depending upon the clafs to which they belong; and that the organized matter of which they are compofed, has very different properties from thofe belonging to dead matter in its natural ftate.

Living bodies, in a ftate of health, are in conftant motion, and are endowed with the power of refifting, to a certain degree, the mechanic and chemical actions of inanimate bodies, as alfo the properties of changing *dead* matter into *organized and living*. They poffefs the power in a greater or lefs degree, of deftroying the fenfible properties of the fubftances expofed to their action, while they preferve their own unimpaired. They protect the animal machine from diffolution and corruption, by acting upon foreign fubftances, and changing them into their own natures.

Thefe various fubftances acquire peculiarities very different from thofe they originally poffeffed. Saline bodies, for example, whether they be alkaline or acid, as foon as they have undergone the operation of digeftion in the ftomach of a living animal, immediately lofe their former properties and chemical characteriftics. The food, whether it be taken from the animal or vegetable kingdom, lofes every trace of its original ftate, and becomes prepared for being affimilated to the nature of the living body; being tranfmuted into a chyle of one uniform nature, however different the fubftances might be from which it was prepared.

But, although they are thus affimilated by the digeftive powers, yet thefe various fubftances differ materially from each other, refpecting their adaption to be transformed into the nature of the animals which confume them; and hence it is, that different animals require different kinds of food, which is alfo the cafe with the fame animal at different periods.

This diverfity in fubftances which are expofed to the action

of living beings, cannot be the effect of a chemical cause; for then would the change produced in the food be constant and uniform; and the chyle, instead of being a fluid of similar properties, would possess those of the nutritive substances from whence it was taken.

Digestion itself is not a chemical but a vital operation, by virtue of which all organized beings are endowed with the power of changing these different foods into their own nature, until it finally assumes the form and properties of that animal by whose organs it was changed. Meat and bread, after they have gone through the process of digestion in a Man or a Dog, are transformed into the specific nature of each. Thus we see numberless plants, nourished by the same water and the same air, assume very different organized forms, which can only be ascribed to a peculiar living power inherent in each.

The effects are totally different whenever disease, or any other cause, destroys the power of living bodies to assimilate substances to their own nature. The food, in such cases, either retains its particular properties, or it undergoes the same changes *in* the body to which it was liable *out* of it. Animal and vegetable bodies, instead of forming of chyle, run into an acid or a putrid fermentation. Saline substances also observe the laws of chemistry, uniting with their corresponding salts. This is illustrated by the effects of magnesia upon acidities in the *primæ viæ*. Whenever the substances taken into the body possess great chemical powers, as the concentrated acids for example, and caustic alkalies, they destroy, by the violence of their stimulus, the organic structure, form a chemical union with its component parts, with which they have an immediate attraction, and it becomes a dead unorganized mass.

How great, therefore, is the difference between dead and animated matter! The first is, in its own nature, *passive*, simply possessing the power of attraction and repulsion; the other is in perpetual action, and produces perpetual changes! "This summit of the Alps, (exclaims the ingenious Ith) these vales, and plains, and lakes, remain in the same state as they were in the days of our remotest ancestors; yet organized beings live, move, and change incessantly: The young become old, the small grow large, one generation arises, and presses upon another. Nothing remains inert, an incessant production and dissolution take place, of beings which, from the first moment of their existence to that of their decay, maintain an uninterrupted course of changes and transformations.

Moreover, mechanic action produces very different effects in dead and in living bodies. Machinery is utterly unable to support itself in a state of uniform action, to supply the losses it sustains by friction, or repair the injuries to which it may be occasionally

occaſionally expoſed. The current of a river wears away its banks; but the action of the fluids in the living body, ſupplies defects, and increaſes the firmneſs of the veſſels through which it circulates. The ſame remark is applicable to chemical powers. Doubtleſs, chemical operations and affinities take place in animal bodies; nay, the experiments made by Galvani, Humhold, and Ritter, render it very probable that all the functions of life are cloſely connected with chemical principles; but, in conſequence of this union, chemical effects are *animalized*, or become *animal* indications; and animal operations can no more be aſcribed to chemical powers, excluſively, than the latter can be referred to the laws of animal life. The matter of heat, for example, is an eſſential and operative power, both in living and in dead bodies; but in its union with living animals, it loſes a part of its chemical properties; it unites and accommodates itſelf to the nature of the living ſubſtance; in a word, it becomes *animal heat*. Electricity is, without diſpute, one of the moſt powerful ſtimulants of the animal frame; but as ſoon as it unites with the living power, it is diveſted of ſome portion of its natural properties, forms a new relation and connection with organized nature, by means of which it follows the laws of animo-chemical nature, and becomes *animal* electricity. Hence it is, that in ſome caſes animal heat and animal electricity deviate from the common laws of Nature, by virtue of their union with animal ſubſtances. For example, it is a common law of Nature, that a body ſhall finally acquire an equal degree of heat with the atmoſphere in which it is placed; but in living beings, this law is expoſed to many exceptions; for, in warm-blooded animals, the temperature of the animal is much cooler than the atmoſpheric air, when it riſes to a high degree of heat; and much warmer when the atmoſpheric air is cold. Thus human beings are as capable of living under the burning ſuns of Senegal, which is ſufficiently hot to boil ſpirits, as in the pinching cold of Hudſon's Bay, by which both ſpirits and mercury are frozen; and the medium heat of the internal parts of the body remains at about 98 degrees of Fahrenheit's thermometer, though expoſed to theſe two extremes of heat and cold. Thus the vital power, and the laws of animal Nature dependent upon it, regulate the influence of the caloric, or matter of heat, in ſuch a manner as to maintain animal heat in its medium ſtate.

We ſubſcribe to the ſentiments of the great Hufeland, as moſt conſonant with appearances, viz. that although the powers of animated matter may be influenced in many reſpects by mechanic or chemical properties, yet they are not to be confounded with them; ſince their laws of action, and their various operations, cannot be explained by any of the principles of

mechanics or chemiſtry which are hitherto known, and appear very diſtinct from the laws which govern inactive matter.

The phænomena of life (as the ſame author has juſtly remarked) may be reduced to two heads. They evidence a *susceptibility* of ſtimulants, and ſpecific mixtures, changes, and formations of organized matter; ſo that the two principal characteriſtics of the vital powers conſiſt in the capacity of a living being to receive impreſſions from ſtimuli, and re-act upon them according to its own laws, which are diſtinct from thoſe of chemiſtry or mechanics; and, ſecondly, in the power of organized bodies to change uſual affinities and combinations in ſuch a manner, that very different affinities and combinations are produced, by which new products, forms, and creations are rendered poſſible; of the exiſtence of which, we can find no traces throughout Nature but in living beings.

The arguments of the Chemical Sect, however ſpecious and concluſive they may appear upon a ſuperficial view, cannot bear the teſt of critical examination, and will prove unſatisfactory to every impartial mind; as the following Obſervations will, we truſt, fully evince.

1. A palpable Induction takes place in their manner of reaſoning; for, becauſe the diverſities in matter in general depends upon a difference in its combinations, they infer that the indications of a particular property in bodies, and, conſequently, their nature, is alone to be aſcribed to the different proportions in the mixture of their component parts; and this being a common law of Nature, muſt alſo be extended to organized bodies; ſo that every appearance in theſe is equally to be aſcribed to different proportions in the combinations of animal matter, and their mutual attractions to each other. This they unphiloſophically conſider as an univerſal rule, and thus apply it to living beings. But the univerſality of this rule is very doubtful, particularly when applied to organized bodies; and, conſequently, as the propoſition cannot be demonſtrated, it cannot be with ſafety applied to living beings.

2. Their arguments are obviouſly founded on a *petitio principii.* Experience, they ſay, teaches us that a ſpecies of matter called animal matter exiſts, and poſſeſſes life; but the grand queſtion whether this organized matter poſſeſſes life *in itself*; that is, alone proceeding from its form and combinations, is paſſed over in profound ſilence. But as a judicious author obſerves, ſince there is not in all Nature any ſpecies of matter known, to which the character of *vitality* belongs, we cannot be authorized to aſcribe this vitality to any chemical action, or combination of theſe dead materials. That matter is capable of various degrees of excellence is allowed, but it does not follow neceſſarily

neceſſarily from hence that this excellence aſcends up to vitality itſelf; much leſs that the vital powers are the neceſſary conſequence of this exaltation of matter alone. This is merely hypothetical, unſupported by the ſmalleſt proof. The extravagance of the ſuppoſition becomes yet ſtronger by the conſideration that although chemiſts, and moreover Nature itſelf, ſo frequently mix very different materials, expoſed to innumerable attractions, and the moſt oppoſite actions both of mechanical and chemical laws, yet they have never produced the leaſt tokens either of organization or of vitality.

3. They explain a propoſition by itſelf, that is, they argue in a circle. According to their doctrine, the chief cauſe of form and combination of organized matter conſiſts in the form and combination of organizing materials. For if figure and organization are only an indication and operation of this matter, we are not to conſider theſe indications and operations as proceeding from figure or organization; for it is an abſurdity to ſuppoſe any thing to be indicative and operative, and at the ſame time to be the cauſe of theſe effects. It is clear to a demonſtration that we muſt have recourſe to ſome other cauſe. Nor can this cauſe be diſcovered in the original difference in the conſtituent parts of organized matter, or in any ſpecific mixture; for theſe, as well as form, are mere *indications*: Therefore the cauſe of vitality, in animal matter, cannot be explained by its form and organization, but it muſt ariſe from ſomething eſſentially different.

4. Though it is acknowledged that ſpecific combinations and organizations, and ſpecific powers, acquire new relations to each other, and that a change in the firſt is followed by a correſponding change in the other; yet we are not entitled to conclude from hence, that the former is the cauſe, and the latter the effect. Such combinations may only be conſidered as a *conditio sine qua non.* A muſician is able to produce various tones from a flute, a violin, or a trumpet; but are we to infer from hence, that this power reſides ſimply and ſolely in the inſtrument? The inſtrument, without the aſſiſtance of foreign aid, is not able to utter the ſmalleſt ſound; in like manner does ſomething ſuperadded to combinations and organizations become neceſſary to produce the effects which the Chemical Sect aſcribe ſolely to theſe.

5. Again, this ſect does not make a ſufficient diſtinction between the firſt principle of life, and the intermediate cauſes of the appearances of vitality in organized beings. They univerſally confound the cauſe of vitality with the cauſes of its operations, and this appears to us to be the ſource of their miſconceptions and erroneous doctrines. They reſt ſatisfied with aſcribing

cribing the whole to organization, without examining in what manner this arrangement of parts, and consequent form, was produced; or what was the original cause. They do not reflect, that the organization and combinations which indicate such wonderful powers, are *effects*, of which the principle of life or vital power, is the first and efficient *cause*; that this last is *active*, and that the inanimate matter upon which it operates is *passive*; so that organic matter, and the organs produced from it, are the instruments of its power, and subject to its laws.

6. Were we to acknowledge that the truth of their hypothesis was fully established, we should not advance a single step in our knowledge of organic powers. It is true, the advocates for the chemical theory are fond of exalting matter; speak much of an affinity approaching to intelligence in the matter composing organized bodies, of specific organized affinities, of animal crystallizations, by which they attempt to explain the theory of propagation and formation of organs; yet, placing the phænomena of animation in this point of view, notwithstanding its novelty, does not enable us to penetrate farther into the subject than any preceding theory: For all these chemical principles have certainly, in their effects upon *dead* matter, decisive tokens; but when applied to the *animal* body, they become unintelligible, and in place of the promised elucidation, present us only with a new phraseology, and new words that explain nothing. We knew before, that matter may possess various degrees of excellence; that mineral substances pass over into vegetable, and these into animal; that in organized Nature the solids are composed of fluids; and that a drop of moisture is the origin of animated beings. The *modus quo* alone remained unexplored. We were ignorant in what manner the bones, muscles, nerves, vessels, so very different in their natures and contextures, could be formed out of the same mass of fluids, the blood. But their chemical explanations leave us as much in the dark as we were before. In what are we advanced by being informed that these various parts are generated by some peculiar relationship or affinity; these fluids *crystallize* themselves into bones, sinews, &c. &c.? The cool Inquirer will not be satisfied with these vague statements, he will persist in demanding, by what power are inanimate bodies, of various natures, united to animal substance? and by what power are they changed into those various component parts which constitute the different organs? How is it that these, which in every other union still remain dead and inanimate, assume, in animal substances, a character totally different, with totally different indications? Surely every unprejudiced person, who

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duly confiders thefe facts, will feel himfelf compelled to acknowledge, that a different principle exifts in organized bodies, which, by its union with inanimate matter, is productive of fpecific organic combinations, and, with thefe, the different phænomena of organic bodies.

[To be continued.]

To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

I Am glad that the Philofophic world have not yet forbid the opinions of one, whofe years might probably bring upon him the appellation *doted.* Advanced age, and the various opportunities which I have had, both of feeing and knowing the world, encourage me to offer a few Remarks upon Philo-Medicus. Perhaps he may be fome Young Phyfician who wifhes well to the Character; but, I fear, he knows too little of the Character, and is fomewhat too faft.

If what follows be deemed worthy a place in your ufeful Journal, it will add to the happinefs of your conftant reader,

AN OLD PRACTITIONER.

Philo-Medicus feems to plume himfelf, that *his defign* has been happily accomplifhed, viz. the publication of an Annual Lift of Medical Practitioners. If it be *happily accomplifhed,* it is more than I can fee. It appears not only that fomething in the *plan* is wanting which is material, and for which I have reafon to believe he has not provided, but it is evident that the plan is very unhappily accomplifhed. "The only difficulty, fays he, that would occur, is in making out the firft lift; after that is accomplifhed, it would become a regular bufinefs," &c. Does he expect that the Medical Practitioners will be at the trouble and expence to fend letters containing their names, &c. poft paid, every year, becaufe it is *his plan?* If not, how will publifhers of Almanacs know when to blot out the names of thofe who have departed this life fince their names were inferted. He cannot expect that the relations of the deceafed will write to the publifhers for that purpofe, great part of them may never know of *his plan.*

Whether it be that I view my own end at no great diftance, and am thereby led to this objection, I will not fay; but it appears

pears to me to be almoſt inſurmountable, as well as others I could point out.

Again, he ſays, " By theſe means every man might be informed of the regular practitioners in his part of the country; and if he afterwards employed an impoſtor, he had himſelf to blame." Now, from the above objection, it is evident the Almanac will be a very uncertain guide; and from the few who have thought it worthy their attention to ſend their names for inſertion at firſt, it cannot be truſted in future as a guide. Numbers of Medical Gentlemen laugh at the plan. What profit can a reſpectable practitioner promiſe himſelf, who has long practiſed in a town with reputation, by having his name in an Almanac? It is only adapted to thoſe whoſe names muſt thus be made public, otherwiſe their names would not be known, or thoſe who are young in buſineſs, and muſt be heard of.

But what can have ſet Philo-Medicus a railing againſt Phyſicians belonging to certain Colleges? If he looks around him, will he not ſee phyſicians belonging to every College who merit not the honourable name? He would do well to remember the old proverb, " Jack will never make a Gentleman," and change it a little, " A Degree will never make a Phyſician." How many phyſicians have I had occaſion to ſee, who, God knows, knew very little of their buſineſs, and would have looked upon a worthy practitioner as contemptible becauſe he was not a Fellow of the ſame College! It would be better for the world if ſterling merit were more prized than nominal honours. I glory to tell, that I have long practiſed under the banner of a Degree from the antient and honourable College of St. Andrews. The Profeſſors are well known to be Gentlemen of learning and probity, and wiſh to confer that honourary reward on thoſe only who deſerve it. They may be deceived. Other Colleges are alſo deceived. Is not an active, ſteady young man, who has ſerved an apprenticeſhip of five or ſix years, and who has ſeen a good practice during that time, well entitled to a degree, after being at college and practiſing for ſeveral years, although not ſo many years at college as is neceſſary to obtain an Edinburgh degree? Inſtead of this, nothing is now more common than to ſee a young man go all at once to College, attend it for three ſeaſons, pop into a druggiſt's ſhop occaſionally, or perhaps not at all, wait on certain gentlemen during his laſt ſeaſon, who tell him what to ſay and how to anſwer upon Examination, write his Diſſertation, and thus procure a degree for him from a moſt reſpectable College. Is not this deceiving the Profeſſors? And can the truth of this be denied? I have known young men made Doctors of Medicine by the moſt reſpectable College, in three years and nine months

from

from the time that they were entirely ignorant of Medicine and Languages. Never tell me of the superlative honour of certain Degrees, until Professors shall require of every Candidate to give oath that he did not attend these gentlemen called Grinders, and that the Dissertation is his own production.

I could easily bring forward a number of respectable learned Physicians, who have their Degrees by means of what Philo-Medicus is pleased to call (being a sweet sounding name) a contemptible Certificate; and in contradiction to his affirmation, I have known Certificates given by Gentlemen of the first character, in order to obtain that honourary reward for those who were well entitled to it by their superior knowledge, and who could not from the time they were at College obtain it otherwise.

From what has been said, however, I beg not to be understood as favouring any abuse in the Study of Medicine, I would be the first to make it public; I only wish that practice were more attended to than speculation, that sense and worth were more esteemed, and that young men would never be too hasty in their opinions.

Scotland, Feb. 7, 1802.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

AMONGST the numerous Papers and Letters the present Controversy upon Apoplexy has given birth to, none has excited my astonishment more than that which appeared in your last Number, with the signature of Dr. Girdlestone annexed; as an Advertisement, to nearly a similar import, had been published in the Ipswich Journal of the 26th of December last, accompanied by another from Mr. Crowfoot; in the latter of which I was charged with having deviated from the *original question*; and in the former Dr. G. in addition to what he has now stated, appeared to support the opinions of Mr. C. that Apoplexy is often a symptomatic Disease, meaning, as I suppose, with his friend Mr. C. that it is generally occasioned by an oppressed or foul stomach, particularly as he immediately afterwards intimates, that the species of Apoplexy arising from extravasation is a *suddenly fatal* disease, and can therefore rarely, if ever, come under the cognizance of the medical practitioner. I saw these Advertisements in the Ipswich Journal, on Sunday morning the 27th of December, 1801, and answered them the

same day, in such terms that I flattered myself would convey conviction to the mind of every *unprejudiced* Reader; that Dr. G. was unnecessarily captious, in objecting to the use of inverted commas to a passage where, if the Printer had made use of *Italics* instead of *such* commas, no objection could possibly have been made. My Reader will, however, be better enabled to judge of my success on this occasion, by a perusal of a Copy of the Letter I allude to, and which I trust, from the established impartiality and candour your Journal has acquired, will find as *ready* admission into your *next* Number, as Dr. G's found in your *last*.

I am, &c.

Halesworth, 8th Feb. 1802. R. LANGSLOW.

A Copy of a Letter addressed to Drs. GIRDLESTONE *and Mr.* CROWFOOT, *written upon the 29th of December,* 1801, *and inserted in the Ipswich Journal, 2d of January,* 1802.

The Letter being already before the Public, and Dr. Langslow's Work on this Subject being expected in a few days, we only give his additional Observations, viz.

I am at a loss to account for Dr. G's motive in transmitting to you a Paper for insertion in your Journal, the *principal* contents of which had been answered previously to the date of it; and it is still *more* extraordinary, that he neither alludes to it, or attempts the refutation of the Arguments there adduced; and as they have never since been noticed by Dr. G. I trust the Public will naturally infer, that what I have there advanced is unanswerable. Be this as it may, I shall not follow his example, but endeavour still farther to explain and account for the *seeming* contradictions which that Letter of Dr. G's contains. Dr. G. at page 149, of your last Number, denies that Mr. Crowfoot's Pamphlet is the joint production of himself and Mr. C. In answer to which, I have to declare *positively*, that it was the general report in this neighbourhood *some weeks* before it appeared, that Dr. G. was not only a supporter of Mr. C's Doctrines upon Apoplexy, but that he was also assisting in the Composition of the expected Work, and correcting and revising the sheets to and from the Press; and there is no person who compares Mr. Crowfoot's first Work, "The Statement, &c." with the second Publication, called, "The Observations, &c." but what will be instantly struck with *the difference of diction and stile*, and must instantly exclaim, these *two* Publications possess the most satisfactory *internal evidence*, that they were *not* the production of the *same* Pen. But this is not all,

all, the Friends of Mr. C. boaſted, that Dr. L. would now meet with his match, or words to that effect, for that an eminent Phyſician had eſpouſed the cauſe of Mr. C. and was aſſiſting in the Compoſition of a Work which was totally to refute what had been publiſhed upon Apoplexy by Dr. L. This was the triumphant language of Mr. C's. friends to thoſe who happened to defend my ſide of the queſtion; and amongſt others, I have to name the Rev. Mr. Bohun, of Bungay, Mr. C's brother-in-law, who, I was informed from *various* quarters, had declared publicly that Dr. Girdleſtone was the redoubted Champion alluded to. And, on this occaſion, I went to Bungay, with the intention of enquiring into the truth of this report, and applied to a perſon in that town of ſtrict veracity, who told me inſtantly, that he had not only heard it from others, but that Mr. B. had himſelf ſtated to him that Dr. G. was correcting and reviſing the Work, and that a ſheet, ſo corrected and reviſed, had been ſent to him, Mr. B. He farther ſtated, that Mr. B. did not mention this fact in confidence, or enjoin ſecrecy, and he therefore conceived himſelf at full liberty to anſwer my queſtions without reſerve, or at the riſk of giving offence. After ſuch clear proofs of the fact, no one, I truſt, can condemn me for believing what I had heard, though I muſt add, that if Dr. G. when he ſo kindly undertook the taſk, did at the ſame time exact ſecrecy, or if Mr. C. and his friends promiſed *not* to divulge it, then indeed it muſt be admitted, they have acted ungratefully as well as treacherouſly, and on this account the Doctor has an undoubted claim upon the pity and compaſſion of the public in this particular inſtance.

The other quotations and anſwers alluded to in Dr. G's paper, I conceive to be completely explained in my letter to Dr. G. and Mr. C. inſerted in the Ipſwich Journal and copied as above, I ſhall, therefore, decline troubling your readers farther at preſent upon the ſubject of Dr. G's paper, and proceed to anſwer Mr. Wilkinſon and Mr. Atkinſon, whoſe papers on the ſubject of Apoplexy appeared in your laſt Journal, and ſhall truſt to your uſual impartiality and candour for their appearance in the *next* Number of your truly valuable Work.

Dr. LANGSLOW, in Anſwer to Mr. WILKINSON, upon APOPLEXY.

When I firſt began the peruſal of this Paper, I conſidered it as an attempt of its Author, to ſupport the Doctrines of Mr. C. on Apoplexy; but when I arrived to near the bottom of page 141, I diſcovered that Mr. W. had *forgot* which of the Diſputants he *firſt* meant to defend; for he there ſays, " The

ingenious

ingenious Dr. Kirkland conſidered that ſort of Apoplexy to which Dr. L. alludes, to be a Diſeaſe, *ſui generis.*" Now, unfortunately for Mr. W. it ſo happens, that, that is the kind of Diſeaſe, *if ſuch exiſts*, which Mr. C. alludes to; for Dr. L. alludes *only* to Apoplexy ariſing from *preſſure upon the brain*, and he admits no other; nay, Mr. W. allows *the Nervous Affection* of Dr. Kirkland, and his converts Dr. G. and Mr. C. to be a Diſeaſe *ſui generis*, *therefore*, I ſhould conclude, *cannot* be Apoplexy; and as Mr. W. ſtates that this *extraordinary* ſpecies of Apoplexy, if Apoplexy it *muſt* be, may originate in the thorax or abdomen, ſurely then, it might with *equal* propriety, and certainly with *greater ſafety*, be claſſed with St. Vitus's Dance, and of a nature *ſui generis*; for by claſſing it with the latter affection, it will ſtill be ſituated amongſt Nervous Diſeaſes, and if erroneous, the error will not be productive of ſuch dangerous conſequences as might ariſe from claſſing it with Apoplexy, otherwiſe a young and inexperienced Practitioner may, upon being called to a Caſe of *real* Apoplexy, heſitate to determine whether his patient is labouring under *real* or *nervous* Apoplexy; and ſhould the latter idea ultimately prevail, he might, by neglecting to abſtract blood, generally and topically, riſk the recovery of his patient. Now Mr. C. did not venture to deſcend below the ſtomach for the origin of Apoplexy, but Mr. W. does not heſitate to include the abdomen; and perhaps the next advocate for Mr. C. may be inclined to extend *the origin* of Apoplexy to the ſphincter ani, or even the great toe! But to be ſerious, Mr. W. next obſerves that Mr. Cheſhire had diſſected many apoplectic patients, in whom he found no morbid appearance in the brain. Ought he not then to have inferred that thoſe patients died from ſome other diſeaſe with apoplectic ſymptoms? And I am perſuaded that many perſons who have expired ſuddenly, have been ſuppoſed to die of Apoplexy, when the heart or ſome other vital organ has been the ſeat of diſeaſe. Nay, Mr. W. ſtates, at page 142, "that in *all* thoſe Caſes related by *his* and *my* worthy Preceptor, Dr. Denman, and called the Apoplexy of Pregnancy, there was *no* extravaſation *upon the brain*, but that *the heart*, in *all* of them, was found *unuſually* flaccid and without a ſingle drop of blood in either auricles or ventricles." Is it not then more reaſonable to infer, that theſe patients were carried off by ſome morbid affection of the latter viſcus, than by Apoplexy, tho' their diſſolution might have been ſudden, and attended with apoplectic appearances? A Work I have now in the Preſs on this ſubject, together with my profeſſional avocations, preclude me attending Mr. W. through the whole of his very long and elaborate Paper; but

but what I have at present omitted to notice, will probably be adverted to, and commented upon, at some future opportunity.

Halesworth, 7th Feb. 1802.

Dr. LANGSLOW, in Answer to Mr. ATKINSON, on APOPLEXY.

Dr. L. has great satisfaction in acceding to the propriety, liberality and candour, which are apparent in *most* parts of Mr. A's paper; and notwithstanding he is at present much pressed for time, thinks it incumbent upon him to devote a few minutes to the examination of *those* points, in which Mr. A. at present seems to entertain opinions at *variance* with those of Dr. L. In page 152, Mr. A. says, "That had there been extravasation in Mr. C's case, is it possible it could be absorbed, though after bleeding, so as to produce an immediate mitigation of every distressing symptom?" Now, the mitigation was *not* immediate, neither were *all* the distressing symptoms removed. The patient had complained the *whole* morning, of pain, oppression, vertigo, tension, and stricture *through her head*, as she expressed it, and which symptoms gradually increased till near dinner time, at which period she was observed to make incorrect and inappropriate answers; this circumstance was particularly remarked by her brother-in-law, who thought from her *unusual* mode of answering him, that he had offended her; she had little appetite, and of course ate but a small quantity at dinner, though immediately afterwards she dropped down senseless, and would have fell to the floor had she not been prevented. Her breathing became laborious and stertorous, her jaw fixed, her pulse full and rather slow, and she remained totally insensible till after the arrival of Mr. Primrose, who upon being questioned by the husband pronounced the case, *I think with great propriety, Apoplexy.* Mr. C. who was the family surgeon, was sent for from a distance of seven miles, but in the interim Mr. P. bled her. The disease being of so alarming a nature, it was determined to dispatch another messenger eleven miles, to request my immediate attendance. Mr. C. however, having a shorter distance to travel, arrived before me, and, as Mr. P. assures me, approved *his opinion*, and what had been done; and then ordered the emetic. Now I learnt from Mr. P. that in about half an hour after the abstraction of blood from the arm, and about two hours from the attack, she *began to recover* her senses, so as to answer questions in *some degree* rationally, though still comatose, and complaining of great pain, stricture, and tension through the head, attended with such sensations

ſations as ſhe never before experienced, or will ever be eraſed from her memory; her reſpiration was ſtill laborious and in ſome meaſure ſtertorous, and theſe ſymptoms remained, ſometimes better, and at others worſe, till after the application of the leeches the following morning, though from the latter operation ſhe received ſpeedy and manifeſt relief. Now, it will I think be admitted by Mr. A. that half an hour was ſufficient time for the abſorbents to remove *such* a portion of the extravaſated lymph, as would admit of *a partial* reſtoration of the ſenſorial energy; it ſurely cannot be requiſite for the *whole* of the compreſſing fluid to be reabſorbed to effect *such* a purpoſe. Allow me for the ſake of argument to ſuppoſe, that in the caſe of this lady, exudation of lymph had begun to take place early in the morning, when the ſymptoms of approaching Apoplexy commenced, and that the firſt tea ſpoonful *(or any given quantity)* produced the head-ach ſhe at firſt complained of, and that the cauſe of exudation ſtill remaining, a ſecond ſpoonful was extravaſated; the pain, as *was the fact*, would be increaſed, together with ſymptoms of ſtricture and oppreſſion, till a third ſpoonful cauſed perhaps vertigo and a confuſion of ideas; the fourth, an increaſe of all theſe, with the addition of a tendency to inſenſibility; and it is probable the fifth may produce the acme of diſeaſe, or ſuch complete compreſſion of the medullary ſubſtance of the brain, as totally to arreſt the ſenſorial functions and bring the patient ſenſeleſs to the floor. Let us ſuppoſe this to be the period Mr. P. was ſent for to this lady. Upon arriving he abſtracts blood from the arm; is it unreaſonable to ſuppoſe the effect of this operation diminiſhed the determination to the brain, and by emptying the veſſels, allowed the abſorbents to act with increaſed power, at the moment the exhalants deſiſted to exude an extra proportion of lymph? And if this is admitted, who will refuſe to allow, that in half an hour they might have reabſorbed ſuch a quantity of fluid as would bring the patient back to the ſame ſtate ſhe was in when the fourth ſpoonful was exuded, as repreſented in the above ſcale, namely, a tendency to inſenſibility, &c. &c. but *with* an *ability* to ſpeak or anſwer a queſtion, as ſhe was not deprived of this function till the fifth ſpoonful was extravaſated; therefore it muſt be allowed reaſonable to infer, that the moment the compreſſing cauſe was diminiſhed by the abſorption of the firſt tea ſpoonful, the effect of the fifth or laſt exuded tea ſpoonful would be obviated, and the patient reſtored to the ſame ſtate of ſenſorial energy which exiſted at the time the fourth ſpoonful compreſſed the brain; and indeed the patient remained under ſuch effects as we ſtated were produced by the ſecond and third ſpoonful exuded, till the leeches were applied the following day, after which,

which, if we may judge from the ſymptoms, the reſidue of extravaſated lymph was rapidly removed. It therefore appears to me, that the grand ſource of the difference of opinion between Mr. A. and myſelf, has ariſen from the difficulty Mr. A. met with in accounting for the *ſpeedy* alleviation of *ſome of the ſymptoms* in this caſe, and his ſuppoſing that it was neceſſary that *the whole* of the extravaſated fluid muſt have been reabſorbed before even *the approach* to a mitigation of the ſymptoms could have been obſerved; and it is ſurely moſt probable to infer, that the laſt ſymptom ariſing from compreſſion of the brain will be the firſt to diſappear as the cauſe diminiſhes.

I formerly ſtated in my Explanation, No. 35 of the Medical Journal, that I did not preciſely accord with Dr. G. Fordyce in his opinion, that Apoplexy rarely occurs from any other ſource but effuſion of *red blood* upon the brain; neither do I think *exactly* with Dr. Cullen, that even expanſion, congeſtion, or fulneſs of the veſſels of the head, will produce *complete* Apoplexy. That a tendency to it, with many of the previous ſymptoms, ſuch as pain, ſtricture, tenſion, coma, and vertigo, may from ſuch a cauſe ariſe, but I much doubt that it will ever completely arreſt the ſenſorial energy, and occaſion the patient to fall ſenſeleſs to the floor. Be this however as it may, it will be ſatisfactory to mankind to know that theſe little hypothetical variations in opinion between Dr. Cullen, Dr. Fordyce, and myſelf, are of ſuch a nature as to require *no* practical difference in the mode of treatment, therefore, ſcarcely of ſufficient moment to diſpute *ſeriouſly* upon. But the Caſe is extremely different with the hypotheſis of Dr. Kirkland, Dr. Girdleſtone, and Mr. Crowfoot; it is of the utmoſt importance to determine upon the *real* exiſtence of nervous Apoplexy, ariſing *without preſſure upon the brain*, as the former aſſerts, and ſeems to be ſupported by Dr. G. and Mr. C. together with the utility of emetics in *any* Caſe of *real* Apoplexy, though in this laſt opinion I do not obſerve Mr. C. to be ſeconded by *any one* of his friends except Dr. G.; and this was the *original* ſource of diſpute between *him* and myſelf. Dr. Cullen, Dr. Fordyce, &c. agree with me in poinion, compreſſion of the brain is the general cauſe of Apoplexy; and I again call upon Dr. G. and Mr. C. to ſtate, *when* and *where* I diſcovered an inclination to relinquiſh the opinion I *firſt* advanced, that when this compreſſion was ſo complete as to bring the patient ſenſeleſs to the floor, it was *always* occaſioned by extravaſation of either blood or ſerum, and generally the latter. This was my firſt opinion, and I have not, as yet, expreſſed a deſire to abandon it. In the treatment of Apoplexy, whether the compreſſing cauſe is blood or ſerum, or turgeſcence of the veſſels of the head, the ſame

remedies will be indicated, namely, to abſtract blood from the ſyſtem, both generally and topically, *in proportion* to the ſtrength, conſtitution, and former habits of the patient, in order as ſpeedily as poſſible to diminiſh the determination to the head, and immediately afterwards to empty the inteſtinal canal by injections, and ſuch laxatives, and ſuch doſes as are not likely to excite the action of vomiting, but on no account whatever to adminiſter an emetic, as ſuch an operation muſt determine the blood more ſtrongly towards the head, and as powerfully impede its return from thence. The farther treatment of this Diſeaſe, together with my ſentiments upon the dangerous doctrine of admitting the Nervous Apoplexy of Dr. Kirkland to exiſt, will be brought forward hereafter.

Mr. A. at page 153, ſtates it to be probable, "that of ten caſes of Apoplexy which are *not fatal*, not above one or two is cauſed by extravaſation of blood upon the brain." Suppoſing this to be the fact, it could only operate againſt the theory of Dr. G. Fordyce; it would in no reſpect affect *mine*, as in moſt of thoſe caſes which terminate happily, I have already ſtated it as my opinion, that the cauſe of the diſeaſe is exuded ſerum. Nay, the caſe which Mr. A. himſelf has adduced at pages 153 and 154 of the laſt Journal, is ſtrongly illuſtrative, and a powerful confirmation of *my* hypotheſis. E. Trinder, (whoſe caſe is ſo accurately related by Mr. A.) there is little doubt, labored under *serous* Apoplexy; and we have ſtrong cauſe to believe that the ſtimulating, joined with the evacuating, powers of the bliſters, contributed to enable the abſorbents to remove a portion of the exuded ſerum, their ſtimulation exciting the abſorbing veſſels at the ſame time that their evacuating property emptied the blood veſſels, and diminiſhed the determination to the head; and theſe good effects were ſo ſpeedy, that we can only ſuppoſe that a *small* portion of exuded lymph, perhaps not a fifth or ſixth part of the quantity extravaſated, was taken up, *before* the good effects became apparent, by enabling her to ſpeak rationally; but it required a *considerable time longer* for the compreſſing fluid to be removed, ſo as to diſſipate *all* the remaining ſymptoms. Is it poſſible for any fact more ſtrikingly to elucidate and confirm the doctrines I have been endeavoring to eſtabliſh, than the caſe here related? That the faculty of ſpeech is commonly the laſt which quits an apoplectic patient, and, upon *my* principle of reaſoning, ought to be the *first* that returns, except where Apoplexy is ſucceeded by Paralyſis, then indeed the reſult may vary, as the organs of ſpeech in that caſe may partake of the latter affection. Mr. A. ſtates, that though E. Trinder recovered her ſpeech in a *few hours* after the bliſters were applied, which was as ſoon as we could reaſonably expect

pect they could produce *any* effect; yet that some days elapsed before the sensorial energy was restored to its pristine vigour, or, in other words, before the whole of the extravasated lymph which compressed the brain was removed.

I cannot, however, take leave of Mr. A. without stating my opinion, that he might have omitted the *concluding* portion of his, in many parts, well written and intelligent Paper, without injury to his reasoning powers, or his liberality as a scholar and a gentleman. His reflections upon University honours, when *regularly* and *fairly* obtained, bespeaks, I am inclined to think, jealousy and envy; and what he means by a long etcetera of Fellowships, Titles, and Diploma Splendours infatuating the multitude, I am at a loss to comprehend, unless it is his intention to join Philo-Medicus in his laudable, well-timed, ingenious, and pointed satire upon such diplomas as are conveyed from the North by mail-coachmen and carriers. It is not, however, unlikely, I may have incurred the displeasure of Mr. A. by stating, that it may be deemed by *some*, derogatory for a Physician to contend with an Apothecary: It is possible Mr. A. might have mistaken my *meaning* in *this* sentence, by supposing I intended to reflect upon the *whole* body; which is certainly erroneous; for when he thinks me writing *contemptuously* to Mr. C. it is *merely* the result of *his* conduct towards me. When a physician is called to a patient, as in the present instance, and happens to prefer the application of leeches to the use of an emetic, will it not be deemed by Mr. A. himself, derogatory for the physician to contend with the apothecary upon the propriety of the one, or the impropriety of the other, whilst *under* the roof of the patient? It is surely, at such a time, the duty of the apothecary to comply with the directions of the physician without a murmur, or an attempt at remonstrance. The moment a physician appears in the sick chamber, he *alone* becomes responsible; and if so, ought he not to possess the *sole* direction of the mode of procuring relief to the patient? However, if the apothecary upon this, or any other occasion, discovers *marked* proofs of defective judgment and skill, he may at a future time, if he possesses talents, ability, and education sufficient for the purpose, detect and expose, with propriety, *such* deficiences in the physician; and it will no longer be derogatory for the physician to contend with the apothecary in clearing himself, if in his power, from such an attack: Nay, was he to decline it, or shrink from it, he would stand self-convicted before the tribunal of the public; and which must have been my own fate, had I refused to answer Mr. C's *first* publication. It was assuredly infra dignitatem to agree with Mr. C. in the house of the patient; but the moment he at-

tacked me *publickly*, it was incumbent upon me to defend myself, and such an act was no longer derogatory. We have, however, but too much reason to believe, that Mr. C. was piqued that the husband of the lady should deem it necessary to require aid, which *he conceived* to be no way superior *to his own*, particularly when the emetic ordered by *himself* was forbidden. His conduct on this occasion, and the numerous misrepresentations exhibited in his first publication, have perhaps rendered my stile of writing more severe and contemptuous than I should have made use of to another apothecary, who might have conducted himself with greater propriety; as I can without hesitation affirm, no one entertains a greater respect for *many*, *very many*, individuals in the Chirurgical and Pharmaceutical Professions than myself: I am acquainted with great numbers, whose acquirements would do honour to the character and title of physician; and to such I shall *ever* think it my duty to behave with that deference and respect they so justly merit. But when *others* conduct themselves improperly, and assert that no person has a right to consult a physician, without *first* obtaining the consent and approbation of his apothecary, it is time for the physician to defend his own rights, and treat such apothecaries with the contempt such declarations merit. — A work I some time since published, has placed this conduct of certain gentlemen in a conspicuous point of view, and, probably, contributed to occasion the present Controversy, which confirms the old adage, That good frequently originates from evil. And I cannot drop my pen, without regretting the anxiety expressed by Dr. Girdlestone to relinquish this important controversy prematurely, and before the utility of Emetics in this disease, and the existence of the Nervous Apoplexy of Dr. Kirkland, are established, or *for ever* consigned to oblivion. Surely, such anxiety indicates fears and apprehensions for the result of the ultimate decision; and which, I hope, will not be relinquished till the desirable object it embraces is obtained.

Dr. L. has now to state, that the Lady whose case has been the subject of the present interesting Controversy, has experienced a *second* attack of the same disease; the progress of it, however, was intercepted before it arrived at its acme by the *timely* application of leeches to the temples and a proper laxative, without any other remedy being indicated. She left her chamber with a *similar* head-ach, stricture, vertigo, &c. and which *progressively increased* till she began to give inappropriate answers, at *which period* the effects of the abstraction of blood from the head became evident, and kept the disease stationary, till the laxative operated; and she was perfectly free from

from disease the succeeding morning. Have we not reason then to conclude, that if she had been bled generally and topically, in the *first* attack, a short time before she became senseless, that such an event would have been prevented, and the disease have been as speedily and as safely removed as in the *latter* instance?

To Dr. BRADLEY.

"Life and Death are objects too important to be sported with in the "manner they sometimes are; nor should the valuable connexions of "our Fellow-citizens be ever sacrificed to the ignorance of the Faculty, "the caprice of a Court, or the artifices of revenge and disappoint-"ment."

Dear Sir,

A Circumstance which has lately occurred, points out the justice of the above observation, which I have quoted from the concluding sentence of the Preface of a very excellent little Work, which, I think, ought to be in the hands of every Medical Practitioner and every Coroner;* and shews how very cautious medical men should be in giving their opinion in cases where life and death may depend on their decision.

On the 29th of December, I examined, in company with Mr. Bartlett, Apothecary of the Finsbury Dispensary, the body of a new-born infant which had been found in a privy. The child was come to its full time, and it was very evident that the mother had had a very tedious and laborious parturition. All the viscera were in a perfectly sound state. On examining the lungs, I found them of a *bright red* colour, and they did not completely fill up the cavity of the thorax. On throwing a portion into water, it rose to the surface; this circumstance, however, had no *weight* with me, as it is well known that many causes may operate to render the lungs of a *still-born* child specifically lighter than water. But the appearance that principally guided me in my opinion on the subject, delivered on oath before the Coroner, was, that of the infant having its hands *very strongly clinched*; I have been several years in the practice of Midwifery, and during that period have delivered many still-born children, but never one in which that circumstance had taken

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* Farr's Medical Jurisprudence, small 8vo. pp. 140. Becket, London, 1788.

taken place; nor could I, by any mode of reaſoning that I could adopt, reconcile it to my ideas how that could happen, unleſs induced by ſpaſm, (except in caſe of mal-formation, which was not the caſe here,) and that ſpaſm could not be brought on, independent of the exiſtence of life. In this opinion I was afterwards in ſome meaſure ſtrengthened, by the authority of Dr. Farr, who, in the work I have already quoted, obſerves, (p. 65,) "We ſhould obſerve likewiſe, whether it has graſped any thing in its hands, *as this is a certain proof of life.*"

Under this impreſſion, I delivered my opinion to the ſame effect, when examined before the *Grand Jury.*

Between this period, and the impending trial of the mother of the child, at the Old Bailey, I determined to have the beſt information that books, and conſultations with profeſſional gentlemen, whoſe anatomical reſearches and enquiries had given them far greater knowledge of the ſubject than any opportunities of my own could afford me; I reflected, that though it might have been born with life, yet the circumſtance of the funis umbilicus being, by the violence of its fall, broke off about four inches from the abdomen, might have occaſioned a mortal hæmorrhage;* although this could not be aſcertained, as the body of the infant had been waſhed clean. I alſo conſidered, that it might have been ſtunned by receiving a blow in its fall down the privy, which the appearance of a contuſion on the ſide of its head rendered probable; and I had my doubts, relative to the completion of the proceſs of circulation and inſpiration, in conſequence of the appearance of the colour of the lungs. I have never had an opportunity of examining the viſcera of a child actually known to be ſtill-born; but well knowing that where breathing and circulation have been perfected, the lungs are of a livid whitiſh colour, and fill up the cavity of the thorax, I ſtated my doubts to ſome gentlemen; and among others, to Mr. Blair,† who not only confirmed my opinion as to the poſſibility that perfect reſpiration might not have taken place, but ſtated a circumſtance which plainly demonſtrated the ground on which I *principally* reſted my opinion of the child having been born alive, to be erroneous. He aſſured me, that not

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* Hæmorrhage does not always take place when the funis is broken off by violence. A lady, one of my beſt patients, was taken ſo ſuddenly in labour, that the child, if I may uſe the expreſſion, *ſhot forth* from her with ſuch force as to ſeparate the funis, which broke exactly in the right place, and as *even* as if it had been cut with ſciſſars; not ſo much as one drop of blood followed, although the child was ſtrong, and very lively.

† As Mr. Blair permitted me to mention his name in the Court, as my authority for changing my firſt opinion, I flatter myſelf he will not be offended at my again taking the ſame liberty in this place.

not long ago, a labour had occurred to him, in which the child, which was ſtill-born, and had been dead four or five days before delivery, had *both* its *hands* VERY FIRMLY CLINCHED.

It is no diſcredit to any man to retract an erroneous opinion; obſtinately to perſiſt in it, againſt conviction, where both the life of a fellow-creature and my own reputation were at ſtake, would be downright murder. In giving my teſtimony on the day of trial, at the Old Bailey, I ſtated the circumſtance related to me by Mr. Blair, (who humanely gave his attendance in the Court) as my reaſon for changing my opinion. Mr. Blair's eminence in his profeſſion, and high reſpectability, had the ſame weight with the Court and Jury as with me, for the Jury were perfectly ſatisfied without requiring that gentleman's depoſition on oath; and as the ſole queſtion then lay in a ſmall compaſs, namely, whether the child was born dead or alive; and as this latter could not be proved, but rather the reverſe, the priſoner was of courſe acquitted.

I have thought it not altogether improper to trouble you with this Caſe, on reflecting how many lives may have been ſacrificed by too haſty an opinion given, or a deciſion made, on grounds erroneous, or not fully conſidered! How many executions have taken place, of mothers for the murder of their infants, from the ſingle circumſtance of the *lungs ſwimming in water!* which no enlightened practitioner will now place any reliance on; but which has been, and may in future be, the cauſe of much miſchief in places remote from the capital, where opportunities of knowing better have not been afforded.

I am, &c.

W. CHAMBERLAINE.

Aylesbury Street, Clerkenwell, Feb. 21, 1802.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

IF you think the following brief Obſervations on the utility of publiſhing Accounts of Epidemic Diſeaſes, worth a place in your valuable Journal, the inſertion of them will oblige a conſtant reader, and may perhaps be a means of increaſing the beneficial effects that have already accrued from its extenſive circulation.

Among the numerous deſiderata in medical ſcience, there is ſcarcely any of greater importance, or which is likely to be attended with happier effects, than an accurate account of epidemic

mic diseases, delivered by an attentive observer, who not suffering himself to be swallowed up in theoretical disquisitions, will be content to relate plain facts, to lay before the public the peculiarity of symptoms and diversities of appearance which he has seen, and to deliver that mode of treatment which he has found most successful. The most valuable labours of the Coan Sage that we possess, are histories of this kind; and similar ones are regarded as the most precious legacies of our excellent Sydenham, and have procured for him the title of the English Hippocrates. But in the present day, when men of learning are so attached to, and dazzled with the allurements of theory, that every thing which does not immediately coincide with their adopted opinion is hastily rejected, this subject, which demands a cool enquiry and impartial judgement, has been universally (I had almost said shamefully) neglected; and since the days of Huxham, Sydenham, and the older physicians, there has scarce appeared in this country, any work on Epidemic diseases. It may probably be answered, that no epidemic of importance has prevailed; but a small Tract, published some time since by Dr. Pearson of Birmingham, and a very recent one by Mr. White of Bath, prove that such affections have existed, and induced me to attribute this silence of our brethen to inattention.

I remember to have seen some time ago in this Journal, a few excellent observations on Hydrocephalus Internus by the latter gentleman, and have now derived great pleasure from the perusal of his Account of the Bilious Fever which has prevailed at Bath for some years back. It adds, I think, much to the author's merit, that he has not indulged in any speculative theory, but contented himself with a plain narrative of facts, told with much perspicuity and judgement, and enriched with much collateral evidence from other writers. The work, however, would in my opinion have suffered no loss, had his observations on the nature of epidemics and some of the cases been curtailed. Nevertheless it is well worth the attention of any medical practitioner, and it presents an excellent plan for any that may be induced to undertake so meritorious a work.

Should these observations chance to arrest the attention of any of your readers, I hope they will deem them worthy of notice; having at the same time candour enough (which I must also request of you) to attribute the poor form in which they appear, to its being almost the first time that any thing has appeared in public, from

Your's, &c.

PHILO-MEDICUS LONDINENSIS.

London, Feb. 9, 1802.

ACCOUNT OF DISEASES IN AN EASTERN DISTRICT OF LONDON.

From the 20th of January, to the 20th of February, 1802.

ACUTE DISEASES.

Typhus Gravior	1
Typhus Mitior	4
Cynanche	4
Pneumonia	6
Peripneumonia Notha	9
Dyſenteria	7
Rheumatiſmus Acutus	5

CHRONIC DISEASES.

Tuſſis	27
Tuſſis cum Dyſpnœa	14
Dyſpnœa	11
Catarrhus	3
Hæmoptoe	2
Phthiſis Pulmonalis	5
Pleurodyne	4
Hydrothorax	2
Anaſarca	7
Palpitatio	1
Cephalalgia	5
Paralyſis	2
Enterodynia	3
Hæmorrhois	2
Vomitus	3
Dyſpepſia	4
Amenorrhœa	12
Dyſuria	3
Lumbago	2
Rheumatiſmus Chronicus	27

PUERPERAL DISEASES.

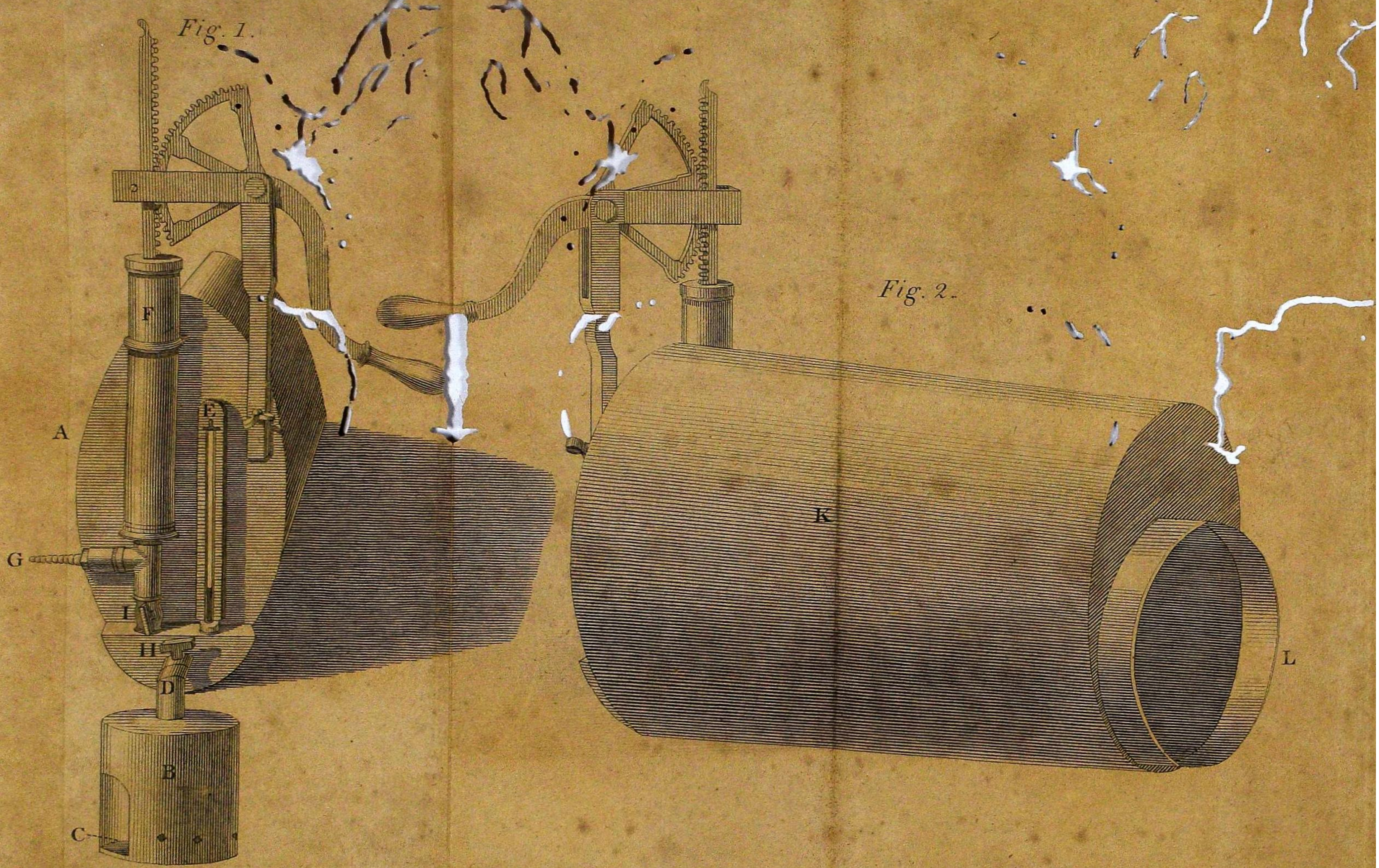
Menorrhagia Lochialis	3
Abſceſſus Mammæ	2
Enureſis	1

INFANTILE DISEASES.

Febris Meſenterica	3
Pertuſſis	7
Diarrhœa	3
Herpes	2
Tinea	1

Since the laſt Report, the weather has undergone conſiderable changes. For ſeveral days the temperature of the air was ſo raiſed, as to produce the feeling and appearance of a haſtily approaching ſpring; and during thoſe few days, ſome relief was experienced by patients labouring under the different diſeaſes of the thorax. Upon the return of cold and froſt, however, the ſymptoms attendant upon theſe complaints have again appeared, and in many caſes have been much aggravated. The diſeaſes referred to have generally prevailed amongſt perſons far advanced in life; and to patients of this deſcription, they have lately proved very fatal: But though they have been conſidered as ranking rather in the ſenile than the juvenile and infantile claſs of diſeaſes, they have lately appeared in ſeveral inſtances amongſt very young children, to whom they have been highly diſtreſſing, and, in too many inſtances, fatal. Peripneumonic affections have, in ſome of theſe caſes, conſtituted the primary diſeaſe; but in others, they ſucceeded a long-continued and ſevere hooping-cough, by which the ſtrength of the patient had been much reduced, and a conteſt with the ſupervening diſeaſe rendered more hazardous. In the former inſtances, or where the peripneumonic ſymptoms conſtituted the primary diſeaſe, the

Smi[torn] Air-pump Vapour Bath.



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R. H. Laurie delt.